THE

AMERICAN JOURNAL OF SCIENCE.

[THIRD SERIES.]

ART. LI.—*On the effect upon the earth's velocity produced by small bodies passing near the earth;* by H. A. NEWTON.

1. THE space through which the earth travels is traversed also by small bodies, or meteoroids. These give meteors of all degrees of brilliancy, and it is reasonable to assume that bodies too small to furnish visible meteors also lie along our path. The impact of these bodies upon the earth, and the consequent increase of the earth's mass, have their effect upon the earth's motions both of rotation and revolution, and hence upon the lengths of the year and the day. The moon's orbit and the length of the month likewise suffer change.

Professor Oppolzer in a paper in the Astronomische Nachrichten (No. 2573) has considered the amount of these two actions and has computed the density which the meteoroid matter must have in the space which the earth is traversing in order to produce upon the relative lengths of the month and the day the observed and unexplained acceleration of the moon's mean motion. This computed density is 59×10^{-18}, the mean density of the earth being unity, and is at least a myriad times, perhaps a million times as great as can be reasonably assigned to observed meteors.* But a body that passes near to the earth has also an action of like character by reason of the attraction of gravitation alone, and I propose to show its amount, for it seems well worth showing that the bodies which pass near us do not have an effect at all comparable with that produced by those which actually come into the earth's atmosphere.

* A proper amount of cosmic dust, if we can reasonably assume that so large an amount of this questionable substance exists, might explain that acceleration.

2. Assume in the first place that the earth is at rest and that a group of small bodies each weighing m' pounds are evenly distributed through space; that they are all moving parallel to each other so as to have a common velocity v relative to the earth on entering the sphere of the earth's sensible attraction; and that there are at first n of these bodies in each cubic unit of space. Let the space considered be a cylinder whose axis has the direction of the motion of the bodies and passes through the earth. Each of these bodies will describe a hyperbolic orbit about the earth, and leave the sphere of the earth's action with the same velocity v with which it entered. By reason of the smallness of the bodies, and their even distribution, the action of the bodies on each other may be disregarded.

3. Because the bodies are assumed to be evenly distributed they will have no resultant action upon the earth at right angles to the original direction of motion. But in that direction of motion the momentum of the whole system of earth and meteoroids will be the same before as after the passage of the bodies. What the meteoroids lose the earth gains. Let the asymptotes of the hyperbolic orbit of one of these bodies make an angle α with its conjugate axis. The momentum of the body on entering the earth's sphere of action will be $m'v$ in the direction of motion. Its momentum in the same direction upon leaving the sphere of action will $m'v\cos 2\alpha$, since the direction of motion has changed 2α. The loss of momentum will be $m'v(1-\cos 2\alpha)$, and the earth gains by reason of the transit of the body an equal momentum in the same direction.

4. Let the perpendicular distance from the earth to the original line of motion of a meteoroid be p. The number of bodies that pass the earth in a unit of time and that have such distance greater than p and less than $p+dp$ will be $2\pi pnvdp$. The momentum communicated to the earth by the whole group in a unit of time will be the same as that communicated through their whole orbits by those which pass the earth in a unit of time, and this will be the integral

$$\int 2\pi pnv^2m'(1-\cos 2\alpha)\,dp$$

taken between the proper limits.

5. The factor $1-\cos 2\alpha$ is a function of p and v. To find its value let p_0 and v_0 be the distance and velocity of the meteoroid at perigee, let r be the earth's radius, and $g=32\frac{1}{6}$ feet. Using feet-second units we have

$$\text{By conservation of energy } v_0^2-v^2=\frac{2gr^2}{p_0}, \tag{1}$$

$$\text{By conservation of areas } v_0p_0=vp, \tag{2}$$

$$\text{And by nature of the hyperbola } p=p_0(\tan\alpha+\sec\alpha) \tag{3}$$

These equations give

$$1-\cos 2a=\frac{2g^2r^4}{g^2r^4+p^2v^4},$$

and substituting in the integral of the preceding article we get

$$\rho m=\int_{p'}^{p''}2\pi pnv^3m'\frac{2g^2r^4}{g^2r^4+p^2v^4}\,dp=\frac{2\pi m'ng^2r^4}{v^2}\log\frac{g^2r^4+p''^2v^4}{g^2r^4+p'^2v^4}, \quad (4)$$

where p'' and p' are suitable limits of the integral, m is the earth's mass in pounds, and ρ is the velocity in feet per second communicated to the earth per second.

6. The value of p' is that value of p which permits the meteoroid to just touch the earth's atmosphere without entering it. Assuming that the radius of the earth including the atmosphere is r' (which may be assumed $=r+100$ miles), putting p' for p and r' for p_0 in equations (1) and (2), we have

$$v_0^2=2g\frac{r^2}{r'}+v^2,$$

$$v_0r'=vp'.$$

Hence
$$p'^2v^2=2gr^2r'+r'^2v^2, \quad (5)$$

and
$$g^2r^4+p'^2v^4=(gr^2+r'v^2)^2$$

Putting also δ for the density of the meteoroid matter if distributed through the space occupied by the group, the earth's density being regarded as unity, and observing that $m\delta=\frac{4}{3}\pi r^3nm'$, we obtain from (4)

$$\rho=\frac{3\delta g^2r}{2v^2}\log\frac{g^2r^4+p''^2v^4}{(gr^2+r'v^2)^2},$$

Or,
$$\rho=\frac{3\delta g\,r'\beta^2}{2rv^2}\log\frac{\mathrm{P}^2\left(1+\frac{\beta^4}{\mathrm{P}^2x^4}\right)}{\left(1+\frac{\beta^2}{x^2}\right)^2}, \quad (6)$$

by putting u for the earth's velocity in its orbit, $v=xu$, $p''=\mathrm{P}r'$, and $\beta^2=gr^2/r'u^2=\cdot069$. The term β^4/P^2x^4 may be dropped as of no account unless x is very small. Substituting numbers we get

$$\rho=\frac{6\cdot83\delta}{x^2}\log\left\{\frac{\mathrm{P}}{1+\frac{\cdot069}{x^2}}\right\}. \quad (7)$$

If the velocity of the meteoroids be that of a comet in a parabolic orbit $x^2=2$ and

$$\rho=3\cdot41\delta\,(\log\mathrm{P}-\cdot034).$$

If their velocity be that of the earth in its orbit

$$x^2=1, \text{ and } \rho=6\cdot83\delta\,(\log\mathrm{P}-\cdot068).$$

In formulas (6) and (7) ρ expresses the acceleration given to

the earth per second in feet per second; δ is the density of the matter of the group if distributed through the whole space considered, the mean density of the earth being unity; x is the velocity, the earth's velocity in its orbit being unity; and P is the radius of a cylindrical stream of meteoroids, the unit being 4056 miles. If the unit for P be taken equal to $4056\left(1+\frac{\cdot 069}{x^2}\right)$ miles, and we use common logarithms, the value of ρ becomes

$$\rho=\frac{15\cdot 7\delta}{x^2}\log_{10} \mathrm{P}.$$

7. Equation (7) may be thus expressed as a theorem.

If a cylindrical stream of small bodies evenly distributed and all moving in the same direction with common velocity shall move past the earth supposed to be in the axis of the cylinder, the small bodies by the law of universal gravitation shall communicate to the earth a velocity along the axis in each unit of time;

(*a*) that shall be proportional to the density of the group;

(*b*) that shall decrease as the velocity increases varying very nearly inversely as the square of the velocity;

(*c*) that shall increase with the radius of the cylinder, and shall be proportional to the logarithm of that radius, measured by a unit that differs from the earth's radius by a small quantity which is a function of the velocity.

Strictly taken, if the cylinder have an infinite radius the velocity communicated is also infinite; but if the radius be comparable with measurable stellar distances, the velocity communicated is not large, since $\log_{10}$ P would then not exceed 12.

8. If we assume the bodies at rest, and the earth in motion through them with a velocity v, ρ expresses the resistance of the system to the earth's motion. Hence, *if infinite space were filled evenly with discrete material bodies at rest, and the law of universal gravitation holds true ad infinitum, there would then be exerted an infinite resistance to the motion of a planet in a continuous right line through the system.* Moreover, a finite system of such discrete bodies at rest constitutes a truly though feebly resisting medium by reason of gravity alone and independently of their impact with the moving planet.

If the small bodies however should constitute an elastic medium the above reasoning does not apply. For equation (6) depends upon the property that the small body enters the sphere of the earth's action in one direction, describes a hyperbolic orbit, and leaves it in another. This may not be asserted of particles of an elastic fluid filling space.

Again, if the planet moves in its orbit about the sun, instead

of in the assumed right line, the integration may not be extended *ad infinitum*. For the earth's motion to and fro would for the very distant meteoroids develop resistances in opposite directions which would cancel each other, the action of the planet upon the more remote bodies not being instantaneous but requiring long periods of time for its development.

9. The action of one of these bodies on the earth may also be looked upon as like an impact. For we may consider the hyperbolic orbit to be replaced by its asymptotes and the whole action of the earth in changing the body's motion to be concentrated at one point, namely, the center of the hyperbola. The reaction of the body upon the earth will be of the nature of an impact in the line drawn to the center of the hyperbola. The combined impacts of all the bodies would have a resultant in the direction of the motion of the bodies.

Again the action may be regarded as though the earth was in motion and the bodies at rest, and that the earth drew the small bodies around as it passed them into its own wake where they exert a greater attraction than they did in front of the earth. This concentration would not take place if the bodies formed an elastic medium.

10. Thus far the small bodies have been assumed to be at rest or moving in one direction with one velocity. Let us now extend our hypothesis and assume that the bodies have all the same absolute speed cu, but that their absolute velocities are directed to points evenly distributed over the celestial sphere, that the bodies are as before evenly distributed in space, and that the earth moves with a velocity u through the system. The velocities relative to the earth will not be uniform nor their directions evenly distributed.

To represent these velocities draw $AB=u$, and about B as a center with the radius cu describe a spherical surface CD. AB will represent in amount and direction the earth's velocity, CB the meteoroid's absolute velocity and AC the meteoroid's relative velocity. There will be two cases, according as A is within or without the sphere: in other words according as c is less or greater than unity. The distinction between these will be considered further on. The meteoroids may be supposed to come from points evenly distributed over the spherical surface CD.

Let the angle $ABC=\theta$, $BAC=\varphi$ and $AC=xu$. Then

$$x^2=1+c^2-2c\cos\theta,$$
$$x dx=c\sin\theta d\theta,$$
$$\cos\varphi=\frac{1-c^2+x^2}{2x}.$$

11. The bodies which move in directions which make angles

greater than θ and less than $\theta+d\theta$ with the earth's motion constitute a fraction of the whole group expressed by $\frac{1}{2}\sin\theta d\theta$. Their united action upon the earth at right angles to the earth's motion is zero because of symmetry. Each meteor's total action multiplied by $\cos\varphi$ gives the effective action along the line of the earth's motion. If we represent as before the density of the whole system by δ, and the earth's retardation per second in feet per second by ρ_1, we have instead of (6) the formula,

$$\rho_1=\frac{3\delta g r_1 \beta^2}{4r}\int_0^{\pi}\frac{\sin\theta\cos\varphi}{x^2}\log\frac{P^2\left(1+\frac{\beta^4}{P^2x^4}\right)}{\left(1+\frac{\beta^2}{x^2}\right)^2}d\theta,$$

whence
$$\rho_1=\frac{3\delta g r'\beta^2}{8rc}\int_{\pm(c-1)}^{c+1}\frac{1-c^2+x^2}{x^2}\log\frac{P^2\left(1+\frac{\beta^4}{P^2x^4}\right)}{\left(1+\frac{\beta^2}{x^2}\right)^2}dx. \qquad (8)$$

Since x does not vary through zero the lower limit must be taken arithmetically positive, whatever be the value of c. The value of the definite integral will be different according as c is greater than unity, or less than unity. The factor $1+\frac{\beta^4}{P^2x^4}$ may be considered equal to unity except in the cases where $c-1$ and θ are both very small and P not very large. The indefinite integral then becomes,

$$\frac{2(x^2-1+c^2)}{x}\log\frac{Px^2}{x^2+\beta^2}+\frac{4(c^2-1)}{x}-\frac{4(\beta^2+1-c^2)}{\beta}\tan^{-1}\frac{x}{\beta}.$$

Hence when $c<1$

$$\rho_1=\frac{3\delta g r'\beta^2}{r}\Big\{\log P+\log(1-c^2)-\tfrac{1}{2}\log((1+c)^2+\beta^2)((1-c)^2+\beta^2)+1$$
$$-\frac{\beta^2+1-c^2}{2\beta c}\tan^{-1}\frac{2\beta c}{\beta^2+1-c^2}\Big\}. \qquad (9)$$

or
$$\rho_1=\delta(k\log P-A),$$

where k is a constant ($=6{\cdot}83$), and A is a function of c. But if $c>1$ we get

$$\rho_1=\frac{3\delta g r'\beta^2}{r}\Big\{\log\frac{c+1}{c-1}+\tfrac{1}{2}\log\frac{(c-1)^2+\beta^2}{(c+1)^2+\beta^2}-\frac{1}{c}$$
$$+\frac{c^2-1-\beta^2}{2\beta c}\tan^{-1}\frac{2\beta}{c^2-1+\beta^2}\Big\}. \qquad (10)$$

Hence $\rho_1=\delta A'$ where A' is a function of c but independent of P'.

12. The results deducible from (9) and (10) may be thus stated as a theorem.

If a widely extended group of small bodies evenly distributed through space have absolute velocities all equal to each other but directed towards points evenly distributed over the surface of the celestial sphere, and if the earth moves *in a right line* through them;

(*a*) A portion of the bodies will come into the earth's atmosphere and affect the earth's motion in the manner of a direct collision;

(*b*) The rest will pass by and exert an effect by reason of gravitation.

If the earth's velocity be *less than that of the bodies* the total effect of the action of the bodies of this second class (*b*) upon the earth's motion will be an exceedingly minute retardation of the earth's motion, even though the extent of the group is infinite. The outer limit of the group, when large, disappears from the expression.

If, however, the earth's velocity be *greater than that of the bodies* the total effect of the action of the bodies of this second class upon the earth's motion will consist of two parts:

First, a very minute acceleration of the earth's motion, depending for its amount upon the absolute velocity of the bodies;

Second, a retardation of the earth dependent for its amount upon the assumed extent of the group. If the action of those bodies which would pass nearer to the earth than a distance P be considered this second part of the action is proportional to the logarithm of P.

That the quantities δA and $\delta A'$ are very small is later to be shown.

13. The change in the form of the expression for ρ_1 as c increases through unity, and its likeness to the abrupt change of the potential of a point relative to a thin spherical shell as the point passes inside the shell is worthy of notice.

The values of A and A′ being independent of P, are dependent upon the inferior limit of the integral (4), which should manifestly be a function of c and θ.

14. In the two forms for ρ_1, viz.,

$$c<1 \qquad \rho_1=\delta(k \log P - A)$$
$$c>1 \qquad \rho_1=\delta A'$$

the values of A, A′, and $k \log P$, can be computed for assumed values of c and P. Thus we get for A and A′

c	A	c	A	c	A'	c	A'
0·4	0·6	0·7	1·3	1·1	2·6	1·4	0·2
0·5	0·7	0·8	2·1	1·2	0·9	1·5	0·1
0·6	0·9	0·9	4·3	1·3	0·4	1·6	0·1

For a distance P equal to the radius of the moon's orbit

$k \log P = 27{\cdot}8$: for P equal to the radius of the earth's orbit $k \log P = 68{\cdot}5$: and for P equal to the distance of a star having a parallax as small as can be measured $k \log P = 168$ approximately.

15. If instead of regarding c as constant we consider a series of groups, some having $c<1$ and some having $c>1$, the total value of ρ_1 made up by adding the several values for the groups, will have negative values of A partly or wholly cancelled by positive values of A′.

If c is a continuous variable in a group, integration takes the place of summation. In particular, if c varies through unity, and there is no abrupt change in the number of meteoroids for which $c>1$ and $c<1$, the values of $\log(c-1)$ and $\log(1-c)$ by which the values of A and A′ become infinite when $c=1$ will near the limit cancel each other.

16. Turning now to the meteoroids that come into collision with the earth, we may regard, firstly, the earth as at rest and without attraction and the meteoroids moving past with a uniform velocity v. In a unit of time the earth would encounter the bodies which would fill a cylinder whose radius is r' and length is v. Their mass $=\delta\pi r'^2 v$, and the momentum communicated to the earth is $\delta\pi r'^2 v^2$.

If the earth's attraction be now considered, the value of r'^2 should become p'^2. But by (5)

$$p'^2 v^2 = 2gr^2 r' + r'^2 v^2 = r'^2 u^2 (x^2 + 2\beta^2),$$

$$\text{and } \rho = \frac{3\delta}{4r^3} r'^2 u^3 (x^3 + 2\beta^2)$$

where ρ includes the effect of the earth's attraction in changing the course of the meteoroids before impact.

17. If we proceed as in sections 10 and 11 to consider a system of meteoroids equally distributed both in locus and in direction of absolute motion, we find for its effect ρ_2 on the earth per second in feet per second,

$$\rho_2 = \frac{3\delta}{8r^3}\int_0^{\pi} \sin\theta \cos\varphi v^2 p'^2 d\theta = \frac{3\delta r'^2 u^2}{16cr^3}\int_{\pm(c-1)}^{c+1} (1-x^2-c^2)(2\beta^2+x^2)dx.$$

This gives for

$$c<1 \qquad \rho_2 = \frac{3\delta r'^2 u^2}{4r^3}\left(1 + 2\rho^2 + \frac{2c^2}{3}(1-\beta^2) - \frac{c^2}{15}\right) = \mathfrak{A}\delta$$

$$c>1 \qquad \rho_2 = \frac{\delta r'^2 u^2}{r^3 c}\left(\beta^2 + c^2 + \frac{1}{5}\right) \qquad = \mathfrak{A}'\delta$$

where $\mathfrak{A}$ and $\mathfrak{A}'$ are functions of c.*

* In his treatment of this question (*Astr. Nach.* No. 2573) Oppolzer seems to have overlooked altogether the absolute motions of the meteoroids.

The computed values of $\mathfrak{J}$ and $\mathfrak{J}'$ for different values of c are

c	$\mathfrak{J}$	c	$\mathfrak{J}$	c	$\mathfrak{J}'$	c	$\mathfrak{J}'$
0·4	445	0·7	513	1·0	609	1·3	723
0·5	465	0·8	543	1·1	645	1·4	764
0·6	487	0·9	575	1·2	684	1·5	806

18. The absolute velocity of the meteoroids that come into the earth's atmosphere must be in general greater than the earth's velocity. For otherwise the number of shooting stars seen in morning hours would be much greater relatively to those seen in evening hours than observations show. Comets and meteors have like orbits, and no known comet would have at a distance unity a velocity as small as the earth's velocity. Therefore the term $k \log P$ that enters into the expression for ρ_1 applies to only a small part of the meteoroids. Remembering this, and also the considerations stated in section 15, we may safely say that in the solar system $\rho_2 > 100\rho$, that is, that the *effect upon the earth's motion of the meteors that come into the earth's atmosphere exceeds at least one hundred fold that of the meteors that pass by without impact.*

ART. LII.—*Sources of Trend and Crustal Surplusage in Mountain Structures;** by ALEXANDER WINCHELL.

TWO facts in mountain structure have baffled, hitherto, the attempts made to arrive at a comprehension of the mechanics of mountain formation. The north-and-south trend of the profounder physiographic features of the earth has no light thrown upon it by any of the orogenic theories commonly entertained. It appears also, from calculations made by Captain C. E. Dutton, Rev. O. Fisher and others, that the shortening of the earth's circumference in cooling from the incrustive stage to the existing temperature would be insufficient to supply the folds and plications wrought into the structure of the mountains. Professor E. W. Claypole has reached a similar result from approximate measurements across a portion of the Appalachian chain in Pennsylvania. These determinations, so far as they are valid, reveal an insufficiency in the contractional theory. Still the mechanical principles of the theory cannot be successfully assailed; and it becomes necessary to seek for some coöperative cause which has hitherto been overlooked. I venture to contribute one suggestion toward the explanation of meridionality, and another suited to aid in the explanation of

* Substance of a communication presented to the Geological Section of the American Association, August 27, 1885.

meridionality and supply also an adequate supplement of crustal surplusage to meet the demands of orogenic phenomena.

1. I assume that meridionality in the earth's surface features, will be granted. There are, indeed many transmeridional features; but they have arisen from geological actions comparatively late. The oldest mountain chains and continental lines tend distinctly toward north-and-south trends; and this predisposition has given direction to many trends of later geological appearance. This is the fundamental impress received by the earth's crust. Evidently, it belongs to a primitive formative stage. We must seek for the cause in the early periods of incrustation.

Now, let us consider lunar-tidal action during those periods. This is a cause to which I appealed in a work published as long ago as 1870, and in periodical literature as early as 1858. Were the moon's tidal efficiency no greater then than at present, its deformative influence must have been experienced by the earth. If ever our planet was a molten sphere, a tidal prolateness stretched its axis in the direction of our satellite; and the axial revolution of the planet changed constantly the portion of matter tidally elevated. As the matter of the molten earth possessed some degree of viscosity, there was then, as always, a *lagging* of the tide, and the moon exerted that action now so well understood, which antagonizes the planet's rotation. After incrustation had begun, this action was not materially diminished. The greater viscosity (or partial rigidity) of the crust would, indeed, tend to shorten the prolate tidal axis; but, in proportion to increase of the index of viscosity; the lagging of the tide would also increase and thus augment the moon's retral action on the tidal protuberance. But the tide continued to rise and fall; and would have continued if the earth had become solid granite. Tidal movements of the crust must result in fractures, friction and displacements.

By as much as the moon's tangential pull on the tidal mass was capable of antagonizing the earth's rotation, by the same it tended to displace the tidal mass toward the west. There must have been some *retral slipping*. The power which could deform a planet could move a raft of frozen matter floating on a molten liquid, especially if floating in the midst of a hemisphere of fragments more or less discontinuous. This westward impulse was continually repeated on each meridian as it came in succession into the position of the tidal crest. The effect was such as could result from a westward push of the forming crust, applied successively over the whole surface within the parallels limiting the tidal disturbance. The slight backward slipping of every part in succession of the tidally moved zone must have developed, in the growing crust, internal structures

meridionally disposed. These must have consisted of incipient meridional ridges and accumulations, meridional belts of greater and less strength, and belts of differentiated internal elements —either in form, position or material.

Now, while refrigerative contraction would necessarily develop wrinkles in the crust, it would not determine for them a meridional trend. This was pointed out by Captain Dutton. The tidal action instanced would, however, produce this result. Nor can it be pronounced insignificant in amount, since evidently, a force which could move the earth on its axis could move a floating patch of the earth's shattered crust.

It will be readily understood that the retral slipping of the tidal mass would be greatest at the crest, as first shown by Professor G. H. Darwin, and would diminish according to a certain law, toward the north and south. As the mean declination of the moon may be regarded as zero, the greatest mean slipping would be on the equator. The sub-meridional pre-dispositions instituted would therefore trend from the equator eastward of north and south.

If, as is probable, the moon's distance were much less during the incrustive periods, the tidal results cited would become more conspicuous. If, as is equally probable, the earth's rotation were correspondingly more rapid, the tidal results would be correspondingly further augmented.

The actions here considered pertain necessarily to the early forming stages of the crust, and have impressed its profounder features. Later, with increased rigidity of crust, tidally formed predispositions were less controlling; and with the growth of oceans, crustal pressures were experienced from other directions.*

2. Meridional trends would be further promoted by the secular subsidence of the earth's equatorial protuberance. That this accompaniment of the slow retardation of the earth's rotary motion must have exerted geological influences was first distinctly shown by Professor J. E. Todd; and I have elsewhere recorded the fact;† but its determinative influence on the trends

* These views, for the greater part, were first propounded by me in printed form in *World Life*, Nov., 1883, pp. 252–255, 350–355; but were taught in lectures several years previously.

† The ample résumé of W. B. Taylor, in the October number of this Journal will not be overlooked. An abstract of this paper was read before the Geological Section immediately after the presentation of my own communication.

I embrace this opportunity to remind the reader that the first conception of the now accepted cause of the moon's synchronistic motions must be credited to Kant rather than Ferrel, as Mr. Taylor thinks. In 1754, Kant presented to the *Royal Academy of Sciences*, Berlin, a memoir entitled: *Untersuchung der Frage ob die Erde in ihrer Umdrehung um die Achse wodurch sie die Abwechselung des Tages und der Nacht hervorbringt einige Veränderung seit den ersten Zeiten ihres Ursprunges erlitten habe.* After appealing to the action of the tides as a cause of diminution of the earth's rotational velocity, he says: "Dieses legt uns auf

of the earth's wrinkles was first conceived by me in the early spring of 1885.* To say that the equatorial protuberance underwent a secular subsidence is to say that the equatorial circumference of the earth, as an effect of retarded rotation, has shortened more than the polar—that indeed the polar circumference has lengthened. That is, the greatest lateral pressure has been experienced from east and west around the equator. An excess of pressure in this direction must develope crustal changes having north and south continuity. Whether the results were foldings or crushings together, or over slippings, their axis-trends would be meridional. This cause then, conspired with early tidal action in predetermining the direction of the longitudinal dimension of the earth's structural features.

3. The same cause produced crustal surplusage around the equatorial zone. Aside from refrigerative contraction of the earth, the equatorial circumference diminished while the polar increased. This cause alone would, therefore, have developed meridional mountain plications over the protuberant belt. If the crustal surplusage resulting from refrigerative contraction was less than existing mountain plications demand, here is a cause which would supplement the supply from that source. Careful measurements may show that the supplementary surplusage needed is not greater than calculations may prove this cause capable of affording. If so, the contractional theory will experience the relief which every physical geologist must have desired, if not anticipated.

4. The meridional predisposition depending on subsidence of equatorial protuberance would be developed north and south of the equator as far as the parallels marking the limits of the protuberance. The meridional predisposition induced by lunar tidal action would be experienced north and south of the equator to the latitude marking the limits of the prolate tidal swell resulting from the moon when over the equator, *plus* the amount of the moon's lunar-monthly declinations. The extent of these actions, therefore, is as great as the actual trends demand; and embraces also, all the strongly plicated portions of the earth's surface.

einmal die Ursache deutlich dar, die den Mond genöthigt hat, in seinem Umlaufe um die Erde immer diesselbe Seite zuzukehren." He proceeds to say that this phenomenon is not due to overloading on the nearer side; that the influence began at the moment when the moon abandoned the earth; that the moon was at first in a fluid state, and that it then rotated with much greater velocity than at present. This historic fact deprives Mr. Ferrel of priority, but does not diminish the credit due him. Kant's memoir was dated at Königsberg, the identical spot where Helmholtz, one hundred years later, put forth the same thoughts as original with him.

* It is but just credit to a sagacious pupil, Mr. W. E. Bond, of Albion, N. Y., to say that he embodied a clear and original exposition of the principle in a thesis presented in June.

ART. LIII.—*The Genealogy and the Age of the Species in the Southern Old-tertiary;* by OTTO MEYER, Ph.D.

[Parts of this article were read at the meeting of the National Academy of Sciences, Albany, N. Y., November 12th.]

PART III.

Reply to Criticisms.

IN Part II of my essay I endeavored for several reasons to be as brief as possible. Such authors and sentences only were quoted as were considered absolutely necessary, saying: "Interesting as it would be, I cannot review here the whole literature regarding this subject," (p. 65). In a few cases it was ventured to condense the result of long researches into one statement. The criticisms* in the October number of this Journal have shown that I was wrong in doing so, and I now propose to add further explanations in consequence of the misunderstandings which have resulted.

Professor Hilgard contests my statements about his work, and says that the stratigraphy clearly shows the succession to be commencing below, Claiborne, Jackson and Vicksburg. In replying to his remarks I shall endeavor to show that I was right in affirming that Professor Hilgard has not proved this to be the true succession. I designate this statement of mine as Statement B. Mr. Hilgard also says that he differs from me "more fundamentally in the sweeping statement" that "only a competent and careful examination of the fossils could indicate the relations of the Old-tertiary strata of Mississippi;" I am therefore obliged also to explain this statement of mine (Statement A) in detail. Professor Hilgard's observations, as far as all-important points are concerned, are published in his "Geological Report of Mississippi;" Jackson, 1860 (beyond referred to as "I"), and "On the Tertiary formations of Mississippi and Alabama;" this Journal, Jan. 1867, pp. 29–41 (beyond abbreviated to "II").

Statement A.

To explain this statement I consider it necessary to proceed in a somewhat methodical manner. We ask at first: can we trace the typical Vicksburg or Claiborne bed over or under the

*1. The Old Tertiary of the Southwest; by E. W. Hilgard, pp. 266–269. 2. Remarks on a paper of Dr. Otto Meyer on "Species in the Southern Old-tertiary;" by Eugene A. Smith, pp. 270–275. 3. Observations upon the Tertiary of Alabama; by T. H. Aldrich, ibid., pp. 300–308.

typical Jackson bed? If two of the beds were connected directly, for instance, by a river, and we could follow the strata foot by foot, then stratigraphy alone could determine their relative age without an examination of fossils. As this is not possible there remains only to determine strata at a distance from a typical locality as belonging positively to a certain period, and then to study the stratigraphical relations to each other or to one of the typical localities. Can we use lithological characters in the determination, of a stratum, whether it was deposited in the Claibornian, Jacksonian or Vicksburgian time? Professor Smith and Mr. Aldrich seem inclined to use lithological characters; but I cannot attribute to them any value, and Professor Hilgard seems to be of the same opinion, for he says (II, p. 30): "A great deal of the obscurity in which the relative age of the Southwestern Tertiary has been involved, is owing to too great a reliance placed by most observers on lithological characters, differences as well as resemblances," and (II, p. 31) "Nowhere has the geologist more need of divesting himself of reliance upon lithological characters, than in the study of the Mississippi Eocene." So we see that we are led to the examination of fossils, and I have only to show now that this examination must be "competent and careful."

Certain fossils may have lived in a single one of three periods only, as far at least as the region in question is concerned. Such fossils are then characteristic of this era. They may be designated here as α fossils. I am not able to name with certainty any example of such a fossil in Alabama and Mississippi.

Another class of fossils, here called β fossils, occur in two or more of these beds, so nearly alike that no differences are recognized and known. For instance, I am unable to discriminate between *Cytherea minima* Lea from Jackson and Claiborne, between *Alveinus minutus* Conr. from Vicksburg, Jackson and Claiborne, and nobody else has as yet published a recognized difference.

A third class of fossils, γ fossils, is numerous in the Southern Old-tertiary. It embraces such fossils as occur in two or three beds in forms, which, though similar, are in some respects more or less different from each other; these differences being in some cases so slight as to be apparent only on the closest examination; in other cases more apparent, and sometimes rather great. For instance, *Venericardia planicosta* in Jackson and Claiborne differ, but the difference is so slight, that I have not applied varietal names. In other cases I consider the differences sufficient to justify the use of a varietal or even a specific name. For examples, see Part I of this essay.

A fourth class, called δ fossils, are such as have been hitherto found in one of the three localities only, with no similar forms

in the other beds, with which they might be confounded. For instance, *Arca Mississippiensis* Conr. in Vicksburg.

Having made this classification, we ask what is the value of the fossils in each of these groups for determining the age of a newly found bed in the region in question. The β fossils are evidently useless for this purpose. For instance, *Cytherea minima* Lea is of no use in distinguishing a Claibornian from a Jacksonian bed, although this fossil, as long as it is not found in Vicksburg, may be considered a δ fossil in reference to the Vicksburgian.

This argument seems simple, and yet my emphasizing it appears to have been in vain. I had to remonstrate in Part II of this essay (p. 63) against Lyell's using, to demonstrate a Claibornian age, fossils which he himself cites on the same page as occurring in Jackson and Vicksburg. Nevertheless, Mr. Aldrich and Professor Smith cite (p. 306 and p. 274) fossils, as for instance, *Rostellaria velata* Conr., *Pecten Deshaysi* Lea, *Turbinolia Maclurii* Lea, *Dentalium thalloides* Conr., *Astarte sulcata* Lea, *Melongena alveata* Conr., *Infundibulum trochiformis*, *Turritella lineata*, found in Jackson as well as in Claiborne, to demonstrate the Claibornian age of a bed. Hilgard uses on p. 269 the genus *Orbitoides* to distinguish from the Jacksonian a bed in Louisiana as Vicksburgian, while he himself has determined fourteen years ago both known species of Orbitoides, and a new one besides, as occurring in the Jacksonian strata of the same State (See Hopkins's Second Annual Report of the Geol. Surv. of Louisiana. New Orleans, 1871; pp. 11, 12, 13.)

A single α fossil, however, would characterize a bed at once. But what are α fossils? They may exist among the δ fossils, but they must not be confounded with them. δ fossils, though hitherto found in only one bed, are constantly being found on each new examination, in the other beds. I have myself found a large number of species at Jackson, which were known before only from Vicksburg or Claiborne. *Venericardia rotunda* Lea, and the allied form *V. diversidentata* Meyer have been known before only from Claiborne and Jackson. Neither Conrad, nor Hilgard, nor I have found this type in Vicksburg. So we might have been tempted to consider this form as a good δ fossil in reference to Vicksburg. Now Mr. Aldrich says (his article, p. 308) that he has it from Vicksburg. There is no end of this. In the most thoroughly explored Tertiary localities in Germany, almost every new examination extends the range of known species, beside bringing to light new ones. There is another reason why we cannot designate any fossil as α fossil. We cannot expect that all the species that lived during the Vicksburgian era should be represented at the locality at Vicksburg. During the same time very different species may have

lived in Alabama, and among their remains may be α fossils. Since, therefore, we do not know α fossils, and since β fossils are useless, there remain only γ and δ fossils for the determination of a bed. A single δ fossil counts for little, but a number of them affords cumulative evidence. Of the greatest importance for our purpose are the γ fossils, since a great number belong in this class, while δ fossils are comparatively scarce. If we find, for instance, in a new bed nothing but *Astarte sulcata* Lea, *Venericardia rotunda* Lea and *Mitra pactilis* Conr., and all these in that form in which they occur in the typical Claibornian bed, we may be justified in referring such a bed to the Claibornian. For such a comparison, however, it is essential that we recognize and state the differences in the forms as they occur in the three beds. If a difference between two γ forms is not recognized, this γ fossil becomes useless for the identification of beds. For instance, if Mr. Aldrich sees no difference between *Venericardia rotunda* Lea and *Ven. diversidentata* Meyer, but says they are alike (his article, p. 307), it is of no use, that he cites *Ven. rotunda* among those forms, which demonstrate the Claibornian age of a bed (p. 306). If we observe the differences, however, these γ fossils (we may speak here of "recognized" γ fossils) are as valuable as δ fossils are.

As the result of these examinations, we can say now the following:

1. There are no α fossils. That is, there exists no single species from which alone we can characterize with certainty a bed as Claibornian, etc.
2. γ fossils which are not "recognized," and β fossils are useless in the determination of beds.
3. Beds can be determined only by δ fossils and "recognized" γ fossils.

Having now collected in Vicksburg, Jackson and Claiborne many species which were not known before, or known only from one of the localities at the time of Hilgard's observations, I have seen, that a number of such species which he, according to our classification, had to consider as δ fossils do not belong to this class and that others exist, which he did not know. Moreover, and this is more important, he did not use in a systematic way the method of "recognizing" the γ fossils. After having worked according to the above method, and with my present knowledge of the fossils, through all his determinations of beds, I come to the conclusion, that some of them are imaginary, many of them may be probable, but no one is fully established. As a positive determination of the beds, however, is the foundation of conclusions I made my statement A: "only a competent and careful examination of the fossils could indicate the relations of the Old-tertiary strata of Mississippi."

I select here three cases, where the determinations of Hilgard are imaginary. The first includes those based upon Zeuglodon. Wherever he finds Zeuglodon or hears that the bones of this animal have been found, he maps the locality as Jacksonian. He thus uses Zeuglodon practically as a Jacksonian *α* fossil, It is not necessary to repeat here what is said about *α* fossils in general, but we may be somewhat surprised to find that, according to our definition, Zeuglodon is even not a *δ* fossil, that is, it is not found among the marine shells of Jackson (see list of Jackson fossils, I, p. 132). What are the facts, then, cited in I, where Zeuglodon is found with other fossils?

1. From observations at Moody's Branch and in the McNutt Hills, Hilgard *deduces* that Zeuglodon bones have been found *above* the Jackson fossils, above but separated only by a few feet (pp. 130, 131).

2. Zeuglodon found with *Cypraea fenestralis* and *Conus tortilis* "and a very large *Pyrula* not seen elsewhere" (p. 134). The first species is a Jacksonian *δ* fossil, but *Conus tortilis* (= *Conus sauridens*) extends from Vicksburg to Claiborne (see part I of this essay, p. 466).

3. Zeuglodon found with *Ostrea*, *Pecten nuperus*, *Scutella*. *Pecten nuperus* (= *P. Deshaysi* Lea) is either a *β* or not "recognized" *γ* fossil.

4. Zeuglodon associated in prairies with "an oyster somewhat resembling *Gryphæa convexa* of the Cretaceous, the vertebræ and teeth of fish and a branching coral (*Eschara* sp.), (p. 128.)

On these facts, so far as I am aware, Hilgard bases the use of Zeuglodon as a Jacksonian *α* fossil in I. In a later article (this Journal, 1866, p. 68), he cites Zeuglodon with three Jacksonian *δ* fossils. I collected and have in my possession parts of the skull, etc. of Zeuglodon associated with the oyster and coral just mentioned, and *Pecten Deshaysi* Lea, between Barnett and Pachuta, Clarke Co., Miss., near the New Orleans R. R.,* I have seen a large vertebra of Zeuglodon in a little street in Enterprise, Miss., that is in a locality which I consider as Claibornian; I do not know, however, whether it was found there, or brought from a distance. I have had sent me from Claiborne the vertebra of a large marine mammal, which I have presented to the Yale College Museum; I do not know, however, where it was found. The whole literature about Zeuglodon cannot be cited here, but altogether I must say that we do not know when and how long Zeuglodon was living in the waters of the South-

* The greatest part of the same specimen had been collected, before I came to this locality. I saw it afterwards in the exhibition in New Orleans (1884), exhibited by the State of Mississippi.

ern Old-tertiary. All localities which are mapped by Professor Hilgard as Jacksonian, based upon the presence of nothing but Zeuglodon, are in my opinion mapped without sufficient reason.

The second case concerns such strata as are determined as Vicksburgian by nothing except *Orbitoides*. Orbitoides has been found in Louisiana in a fauna determined by Hilgard as Jacksonian. I found moreover a specimen of Orbitoides in stratum *e* and one in stratum *b* in Claiborne.* Orbitoides is a β fossil from Vicksburg to Claiborne, and this Vicksburgian determination is therefore without any foundation.

The third case refers to the strata north of Enterprise, mapped by Hilgard as "siliceous Claiborne." From this whole large formation he cites altogether only two species, *Venericardia planicosta* and "*Cardium Nicolleti?*" (I, p. 124). The first species is known from Claiborne and Jackson, the second from Jackson. Therefore I am of the opinion that this formation north of Enterprise is determined as Claibornian without a shadow of reason.

Statement B.

Hilgard condenses his proof of the succession in the sentence printed in italics:† "Going down streams flowing to the southward, the strata successively sink below the water's edge as the observer progresses, in the order Claiborne, Jackson, Vicksburg, Grand Gulf, as identified by their leading fossils." This is based mainly upon two profiles in two different meridians, the first through Jackson along the Pearl river, the second along the Chickasawhay.

The proof rests on two criteria. The first is that all the strata are determined as Claibornian, Jacksonian and Vicksburgian in a reliable way; the second is, that they have the stratigraphical relation which Hilgard here claims, that is, that the dip in his two profiles is not only south, but nothing else than south. Now we might stop and say: the first criterion is not correct, we cannot rely upon his determination of the beds, consequently his deductions from their stratigraphical relation have no value. This is a fact; but I wish to show, besides, that the second criterion is even worse than unreliable, that it contradicts Hilgard's own observations.

In I, Hilgard states that the dip of the strata in Mississippi is south, with one exception. This exception, however, refers just to one of our profiles. In I, pp. 128, 129, he explains, that the dip from Jackson to Canton is north. He calls this

* Part II, of this essay, pp. 69, 70; see also the remark below.

† His article, p. 268.

"some irregularity" (p. 128). But on our second profile, along the Chickasawhay, he mentions only southern dip. In II, however, he says, p. 37, that going southward, he finds an anomalous reappearance of older strata in *two* different meridians. "One of the cases is noticed in my report (p. 128). From Jackson to Canton, etc." "*The other case occurs on the Chickasawhay, contrary to the statement in my report* (*l. c.*), *in making which I overlooked some specimens and notes of* 1855, *then mislaid.* Then he describes how Jackson strata "dip up" again at the most southern part of the belt of the marine Tertiary. Further he continues (p. 38): "*So far, I have been unable to observe the marine Tertiary in juxtaposition with the Grand Gulf group on the Chickasawhay and cannot positively assert that the former dips under the latter at all.*" These statements, so far as I am aware, Prof. Hilgard has not withdrawn in later publications and therefore I cannot imagine anything else, than, that in penning his last article against my theory he has entirely forgotten his former publications.

Before continuing, I give now a general profile across the marine Tertiary of Mississippi and Alabama, based on these observations of Hilgard and on my former researches.

GENERAL PROFILE ACROSS THE SOUTHERN OLD-TERTIARY.

A', A'', Lignitiferous strata with gypsum, without Tertiary shells; (A', Eolignitic, A'', Grand Gulf); V, Vicksburgian; J, Jacksonian; C, Claibornian.

According to this profile the waters of the Tertiary formed a belt in the direction E.S.E. to W.N.W. (as far as Alabama and Mississippi are concerned). Their deposits *overlie* the A'', that is, the Grand Gulf group. If this profile is right, all existing maps are wrong, since they represent the marine Old-tertiary as underlying the Grand Gulf, which is usually colored as Miocene (see the maps of Heilprin and McGee); while I think that, with perhaps the exception of some local fresh-water formations, the Grand Gulf group is older than the marine Old-tertiary. With this profile I need not explain why in the northern part of the belt the dip is everywhere south, in the southern part, however, becomes north, and why older strata reappear when we go south.

This phenomenon, observed twice by Hilgard in Mississippi, was also observed by Tuomey in Alabama. I cite here Hilgard, II, pp. 36, 37: "In the general (north and south) section

accompanying Tuomey's geological maps of Alabama, the Tertiary strata are represented as dipping southward, conformably with those of the Cretaceous. Nevertheless, in the section from Baker's Bluff to the lower salt works on the Tombigby, he finds the white limestone (=Jackson and Vicksburg groups) occupying 'a trough-like depression in the Buhrstone formation.' In conversations with me, a few months prior to his death, he expressed his belief that such was the general disposition of the Tertiary strata, and that on close examination it would turn out that the strata passed over in going southward from the border of the Cretaceous, would be again passed over in reversed order still farther south. My report of the existence in Mississippi of a lignitiferous formation (the Grand Gulf group) southward of the marine Tertiary seemed to confirm this view." We see that Tuomey before his death came to the same opinion about the stratigraphy of the Southern Old-tertiary that is represented by the above profile.

How does Hilgard now explain this "anomalous reappearance of older strata"? He finds the cause of it in local upheavals. In reference to Jackson, he says (I, p. 129): "It seems difficult to account for this condition of things unless by supposing a local upheaval of the underlying formation to have taken place before the deposition of the lowest of the Jackson stage." In II, p. 38, he explains the other two facts in a similar way: "Similarly, if between Baker's Bluff and the Salt Works on the Tombigby, or between Dr. Miller's and Red Bluff on the Chickasawhay, the sea bottom had a slight trough-shaped undulation (such as the ocean beds of our time frequently exhibit) the existing state of things would result." We see that Hilgard has to use the hypothesis of local undulation in each of the three sections which are studied, and each time at the southern part of the belt. This is not probable, but it is possible.

There are, however, other observations of Hilgard, which agree with my cross-section, but which force him to more hazardous hypotheses. As already cited, he has not observed the juxtaposition of the Grand Gulf group with the marine Tertiary on the Chickasawhay. He continues, however (II, p. 38), that, "in Hinds, Rankin and Smith counties their relative age is clearly exhibited." From these words we might expect that he has observed there the Grand Gulf beds overlapping the marine Tertiary, especially the Vicksburgian. I cannot find any place in Hilgard's publications where this is actually proved. On the contrary, in the neighborhood of Brandon he finds the lignito-gypseous beds of the Grand Gulf group *overlaid* by limestone with Orbitoides (I, p. 138, II, p. 40). He

does not make now the simple conclusion, that the Vicksburgian overlies the Grand Gulf, but he argues: as the Vicksburgian overlies these strata, they cannot belong to the Grand Gulf, they must belong to the Vicksburgian. These strata have precisely the characters of the Grand Gulf group, and Hilgard says expressly: "*and so I referred them until I found them overlaid by a string of limestone nodules containing Orbitoides, about forty-five feet above the uppermost sands of the Vicksburg group.*" Limestone with Orbitoides is certainly marine Tertiary and is considered by Hilgard always as Vicksburgian. Therefore I fail to see not only why he does not conclude from this observation that the marine Tertiary overlies the Grand Gulf, but also why he does not mention it as a case where the Vicksburg group "dips up," or has a northern dip, since this follows from his statement of the heights.

If Vicksburg is at the top of the marine Old-tertiary we should expect to find below it the Jacksonian and Claibornian. We cannot expect to find exactly the same species as at Jackson and at Claiborne; for during the Jacksonian and Claibornian time a different fauna may have lived in Vicksburg, consisting perhaps of a few species only. But what we have to expect is to find at least marine Tertiary. What is, however, the base of the Vicksburg Tertiary at Vicksburg? I have not observed it, but according to Hilgard, I, p. 141, there are below my "Lower Vicksburgian" at first "25 feet of gray or black lignitic clays or sands with iron pyrites, exuding salts and sulphuretted hydrogen." Then "3 feet solid, lustrous lignite, with whitish cleavage planes," and then follows the top of a limestone without fossils. This is what we should expect according to my profile—lignitiferous strata without marine fossils; even the salt, which according to Hilgard (I, p. 148) characterizes the Grand Gulf, is not wanting.

What is under the Jacksonian at Jackson and neighborhood? We find (I, p. 123): "In the wells bored in South Madison by the Rev. Mr. Lambuth, the fossiliferous marine strata of the Jackson group were passed through at about 90 feet, after which 'blue dirt,' with selenite, several ledges of sandstone and a lignite bed of 40 feet thickness were struck, but no more marine strata were reached at a depth of 415 feet." We see here that the non-marine lignitiferous strata, which crop out at a few places in Vicksburg, appear here near Jackson, but north of it, in a depth of 90 feet, and then have a thickness of at least 325 feet. Hilgard continues: "At Jackson, however, at the Penitentiary well, after passing through 32 feet of surface material and fossiliferous strata of the Jackson age, lignitic clays were penetrated for 418 feet, after which a bed of shells

20 feet thick, extremely rich in greensand, was passed through into water-bearing sand. The friable shells brought up by the auger are too much comminuted to allow of determination. Whether this led to a continuation of the Shongalo deposit or an independent basin or estuary; there can be little doubt that it also is of the Claiborne age." So we see that the non-marine lignitiferous strata below the marine Tertiary have a thickness of 418 feet beneath Jackson. Hilgard's last sentence gives us his explanation of these phenomena, precisely expressed on p. 107: "The several marine stages are in most cases separated by intervening strata of dark colored, often lignitic clays, as above mentioned; moreover, both the base and the top of the Older Tertiary are formed by strata of this character of considerable thickness." Now it seems probable that, especially in the lower part of the marine Tertiary, lignitiferous beds of moderate thickness may appear between the other strata; but what Hilgard advances here looks to me extremely improbable. A Jacksonian of 30 feet, separated from a Claibornian, of which no shell is determined, by 418 feet non-marine lignitiferous strata appears to me a hazardous hypothesis. In the same way Hilgard is obliged to accept that the Vicksburgian is separated from the Jacksonian below it by the same kind of strata of unknown thickness. He might reply: the thickness of these lignitiferous strata in Vicksburg is only 28 feet and then follows a Jacksonian limestone (I, p. 141), but what fragment of a fossil has he for such a determination of this limestone?

The most inexplicable of all things for Mr. Hilgard, however, must be and is the fact, that in his Grand Gulf formation, which surpasses greatly in extent the whole marine Tertiary in Mississippi, not a fragment of a marine shell has been found (see I and II, pp. 40, 41). II, p. 40, says: "Neither Wailes, who resided among them . . . . nor myself, who have delved in scores of exposures, have ever found a trace of any fossil whatsoever." If the Grand Gulf group originated by a rising of the coast *after* the time of the Old-Tertiary, why has not a single Miocene shell been found in this enormous "Miocene" territory? In II, p. 41, Hilgard attempts an explanation. "Should the chain of the Antilles, after the close of the Eocene epoch, have for some time cut off the Gulf of Mexico from the Atlantic, it seems possible that the deposits of the former might have changed their character to the extent required by the facts observed. A strong influx of fresh water—perhaps that pertaining to the Great Lignite era—from the continent might for the time being have extinguished the Eocene marine fauna without replacing it by another sufficiently numerous to be

readily detected in the deposits of the period, which might thus correspond to the Atlantic Miocene. Upon the subsequent irruption of the Gulf Stream through the Antilles chain, the formation of normal marine deposits along the margin of the Gulf would be resumed."

This and the other hypotheses, to which Hilgard is forced by his observations, appear to me so surprising, that I found no other explanation than that which was expressed by my statement, that he accepted the theory of his predecessors as a proved fact. This statement is also attacked with much warmth by Hilgard in his article, but I do not see any reason to withdraw either this or my other statements about his work. In one case, however, Hilgard protests with reason against one of my sentences (p. 65): "Therefore Hilgard made this belt make a sharp curve around Jackson." This may sound, as he understands it, as if he did not find the Vicksburgian south of Jackson, where he maps it. This I do not wish to say. I withdraw the sentence and replace it by the following: "Therefore Hilgard had here at least to look for a deviation from the regular straight line."

The only (marine?) shells, so far as I am aware, that are known in the Grand Gulf group, were observed by A. Bigelow * in Alabama. In the lower part of "a sandstone formation, which is quite extensive in the southern part of that State," he observes "very obscure impressions of shells, apparently all bivalves. There are evidently several genera; the outlines of some are quite regular, and in two or three a part of the hinge is discernible." This does not sound like Miocene. Bigelow continues on the next page: "It is questionable whether impressions of shells can be found in any other place than the one I have mentioned." He finishes his article: "The age of this sandstone I am unable to determine; I hope to have the opportunity soon of submitting my specimens to some one well acquainted with fossils." It is to be regretted that Bigelow seems to have found no such opportunity. Can any one give any information in regard to these fossils?

The Grand Gulf in Louisiana is described by Hopkins.† I fail to see in his report any *proof* of an overlying of this group over the Vicksburgian. On the contrary, *at the southern part* of the marine Tertiary belt, in Catahoula parish (p. 16), he states that *a bed* containing *Corbula alta overlies* lignitic beds, and gives an explanation of this fact similar to that of Hilgard. Hopkins's explanation of the absence of fossils in the Grand Gulf is the following (p. 20): "It is, that the water must have

* This Journal, II, 1846, ii, pp. 419–422.

† Second Annual Report Geol. Surv. of Louisiana, 1871, pp. 18–21

contained too much salt for the fresh-water species to live in it, and too little for the marine, and must have been subject to great fluctuations in freshness by the influx of water in wet seasons, and its evaporation in dry, to exclude species that can live in brackish water." From the *advanced state of erosion of the Grand Gulf group* under its cover of drift (pp. 20, 21) Hopkins concludes that at least during the Pliocene time this group must have been dry land. On p. 18 Professor Hopkins says: "A rather sharp series of escarpments marks the northern boundary of the Grand Gulf." These are perhaps old shore-lines.

Professor Hilgard determined a certain portion of Louisiana territory in Landry parish *within 70 miles of the Gulf coast at first as Grand Gulf, then as Cretaceous.** In another article† he says: "While the Vicksburg rocks show at all long exposures a distinct southward dip of some three to five degrees, the position of the Grand Gulf strata can rarely be shown to be otherwise than nearly or quite horizontal on the average; although in many cases faults or subsidences have caused them *to dip, sometimes quite steeply, in almost any direction.*" Why have these "Miocene or Post-miocene" disturbances left the Eocene strata undisturbed?

Interesting as it would be to discuss more fully the Grand Gulf and Eolignitic, brevity again obliges me to turn to Professor Smith and Mr. Aldrich. I myself described limestone in the profile of Claiborne above the proper Claibornian, stratum "*e*." Therefore Professor Smith might have omitted everything by which he attemps to show the same thing, that is, that this stratum is sometimes overlaid by limestone. His article and the corresponding part in that of Mr. Aldrich, however, attempt to show, moreover, that *Orbitoides* and *Spondylus dumosus* occur above "*e*." Professor Smith as well as Mr. Aldrich believe that they have proved this beyond doubt. While still questioning it, I will here not dispute it. It follows that no conclusion can be made either from *Orbitoides* or from *Spondylus dumosus*, as they occur above as well as below stratum "*e*." I fail, however, to find anything in Professor Smith's article that proves the fauna in Jackson or Vicksburg in Mississippi to be more recent than that in the stratum "*e*" in Claiborne in Alabama, and hence say nothing further on this point.

As to three charges made against me I add a few words: I indicated some words in a quotation from Lyell by . . . . , by dots. Professor Smith says that I thus carefully omitted the proof of Lyell. I carefully scrutinized these words before

* See this Journal, Nov., 1869, p. 343. † This Journal, July, 1881, p. 58.

using dots, and dotted them because if they had been cited, it would have been necessary to show the reader that they did not prove anything at all. I should have been obliged to show how Lyell fails to give the proof of this statement here or afterwards, how no author in the following literature proves this statement about this locality, and how Tuomey, a good observer, knowing the strata of Alabama better than anybody before him and accepting all conclusions of Lyell, flatly contradicts this statement by saying: "It must be recollected that the Claiborne fossiliferous bed is nowhere in absolute juxtaposition with the overlying *Orbitoides* limestone," which words I cited afterwards (p. 64).

On page 273, Professor Smith says that I did not quote an article of Hale; and Mr. Aldrich also refers (p. 306) to the same apparent neglect. As I mention Mr. Hale's name on page 69, in the fourth line, it was sufficiently manifest that I knew of his published paper, and since I say that I do not attempt to review the whole literature, they should have inferred that I found nothing of sufficient importance in Mr. Hale's article to require a further mention. As attention has thus been drawn to Mr. Hale's article, I will here state that, although it is not directly connected with our subject, one fact which he mentions is of the highest interest and has impressed me for a long time. On page 357 and 358 he speaks of a terrestrial vertebrate fauna, found as far as I can understand him, in the proper Claibornian stratum. A cranium from this fauna in his cabinet he compares with Glyptodon. I have not found anything about this in the rest of the literature. Can anyone give information about this very remarkable fauna and tell where the specimens are?

One of my main purposes in Claiborne was to keep the fossils of the different strata separate from each other. Therefore the suggestion of Professor Smith, that my specimen of Orbitoides in "*b*" (and in "*e*") and Conrad's *Spondylus dumosus* might have been washed down from the higher strata above, is not very flattering either to me or to Conrad. Conrad says, that he found his specimen attached to an oyster in the resp. stratum. So this oyster must have been washed down also. But I fail to see *d priori*, why Professor Smith suggests this possibility. As yet no observer has found a single specimen of these two species in the strata above "*e*" in the Claiborne profile. Therefore, as far as we know from observations, they do not occur there. How, then, could they be washed down? As for *Spondylus dumosus*, moreover, Mr. Aldrich states (p. 305) that he has "lately found it at Hatchitigbee bluff, 25 feet beneath the buhrstone," that is still lower beneath the proper Claibornian than Conrad found it.

Mr. Aldrich says, p. 303: "Now if Dr. Meyer's theory is correct we should find underneath the Claiborne beds the Jackson and the Vicksburg formations."

He then proceeds to show that several hundred feet of marine Tertiary occur beneath the Claibornian, while no one has demonstrated the existence of such strata below the Vicksburgian. Does now Mr. Aldrich expect that all those species which lived during the Vicksburgian era at Vicksburg and these alone lived also at every place in Alabama? I explained that the fauna of Wood's Bluff, a locality far below the Claibornian, contains entirely new forms or β fossils, which two classes do not indicate an age. It contains also a few Vicksburgian (one Jacksonian) δ fossils.* This is as much as we can expect according to my theory, and I gave this as my "reason 6." Mr. Aldrich seems now to have the following opinion about the range of, for instance, that variety of *Pleurotoma terebralis*, which Conrad calls *P. cristata:* this variety occurs in the lowest part of the whole Tertiary (Wood's Bluff and lower); then follow 700 feet without it, then the whole Claibornian and the whole Jacksonian without it and at the top we find the same variety again in the Vicksburgian. While according to my theory this form occurs probably only in the Vicksburgian. Mr. Aldrich misunderstands my reasons 3 to 6 so thoroughly that it is perhaps useless to explain them at length and to compare this explanation with what he says about them. Three points only may be mentioned.

There may be two different opinions about the relation of the two varieties of a γ fossil: 1. They have a common near ancestor. 2. They are connected by direct descent. As all agree that the Vicksburgian, Jacksonian and Claibornian was deposited in different times, I think that the second opinion is the best explanation. This—that this opinion is the most probable—is the theory advanced in the first part of this essay. Professor Smith calls this (p. 270): "inferences derived from what he thinks should have been the course of evolution." He as well as Mr. Aldrich think now that I base my reasons 3 to 5 upon this theory. Only reason 5 is based upon it. Mr. Aldrich says that the argument in reason 5 "passes his understanding." This is not my fault, it is not my duty to explain to him the "biogenetische Grundgesetz." Reasons 3 and 4 are general methods, independent of direct or indirect descent. In reason 7 I cite the observation of an author and say what conclusions I make from them. Mr. Aldrich need not accept my conclusions if he thinks they are wrong, but he has no

* Mr. Aldrich's mention of *Natica Mississippiensis* Conr. from Wood's Bluff (p 308 corroborates Professor Heilprin's determination of this Vicksburgian fossil.

right to say: "Here Dr. Meyer is guilty of assuming a parallelism which he has not seen." On p. 305 he says: "if they are below this then Dr. Meyer has to sandwich in between Claiborne and Jackson the whole of the Buhrstone formation." Mr. Aldrich misunderstands me again. I do not and I need not accept such a theory of "sandwiches" of several hundred feet thickness, as Hilgard is obliged to do and Mr. Aldrich must do with him, if he wishes to maintain the old theory. To avoid further misunderstandings I give here a table of the strata in Mississippi and Alabama. As far as the Red Bluff is concerned, I accept Hilgard's determination (I, pp. 135, 136), as this is apparently corroborated by my own observations on fossils.

	Miss.	Ala.
Claibornian.	Enterprise?	The Claiborne profile.
Jacksonian.	Jackson, (Moody's Branch).	Buhrstone, etc.
Vicksburgian.	Red Bluff. Higher Vicksburgian. Middle Vicksburgian. Lower Vicksburgian.	Wood's Bluff and strata below.

Grand Gulf = ? Eolignitic.

ART. LIV.—*The Condensing Hygrometer and the Psychrometer;* by HENRY A. HAZEN.

[Read before the Washington Philosophical Society, Oct. 24, 1885.]

MUCH has been written and said against the use of a condensing hygrometer for determining accurately the moisture contents of the air. The following is a recent example: Mr. R. Strachan of England, writes as follows to Symons's Meteorological Magazine for June, 1885: "A condensing hygrometer, whether Daniell's, Regnault's, Dines's or Alluard's, has the thermometer's bulb immersed in a cooling medium, and one surface of the dew plate is also in contact with the cooling medium, but the surface upon which the dew is formed is cooled by conduction and is exposed to the air which may be many degrees, 50 or 60, or more, warmer. In these circumstances, when dew appears, the thermometer must be colder than the outside of the place. When the dew disappears the thermometer cannot have

received the same addition of heat as the outer surface of the plate." There seems to be a slight misconception by the writer of the above as to the amount of the effect under these conditions. It is an undoubted fact that in the hands of an unskilled observer the results will be exceedingly faulty, but if due attention be paid to the elimination of errors, a fairly constant determination of the dew-point may be made again and again and under widely differing conditions.

The psychrometer has received even greater condemnation than the instrument just alluded to. The following is quoted from Symons's Magazine for May, 1885. p. 56, and is an extract from a letter by Mr. Dines: "We have for many years past relied almost exclusively on the indications of dry and wet-bulb thermometers to give relative humidity; it is now admitted that 'all deductions from these are open to doubt.' I should go further than this and say that they ought not any longer to be used for that purpose. It is only too true to say that, out of the mass of observations which has been accumulating for the last few years, we have no data upon which we can rely to ascertain to what degree of humidity the air attains in this country, and which is of far more importance, to compare the dryness of the air at one place with that at another; even at times when both thermometers read alike it does not follow that the air is saturated with vapor, or in other words that its relative humidity is 100 per cent. Objections have been made to the form in which the figures composing a table of relative humidity are given, but this is as nothing when compared with the manner in which those figures are obtained. I should be sorry to say one word against the use of the dry and wet thermometers, but instances by the thousand show plainly that they are not fitted to give the dew-point temperature, and it is to the persistent use of them for this purpose, that Mason's Hygrometer, one of the most simple and useful of all meteorological instruments, has been brought into discredit." Here again there is great misunderstanding; it is granted that, as ordinarily observed, the psychrometer, in a very close shelter or on the north side of a building where there is no ventilation, will give exceedingly unsatisfactory results, but that is not the fault of the instrument. It is hoped to show that with proper care the indications of the psychrometer may be made very accurate, and, what is more important, may be made nearly invariable, i. e., so that constant results may be had under the same conditions. Unfavorable criticisms like the above might be quoted sufficiently numerous to fill a small volume.

It would seem that, if a nearly uniform law can be established between the indications of these two instruments, under all conditions of temperature, dryness, motion and other con-

ditions of the air, certainly we can depend upon the indications of each by itself at all times. It is the object of this paper to indicate a practical method of using these instruments, to contribute toward the establishment of a law controlling them, and to show what accuracy may be attained by either of them. The importance of a practical discussion of this question will be readily recognized. In many instances weeks and even months have been spent in an endeavor to obtain satisfactory comparisons. A most significant sign of the present state of the problem is to be found in the fact that the International Meteorological Committee which has in charge the duty of computing meteorological tables have abandoned the project of preparing tables for the psychrometer. When we add to this the fact that large sums have been expended in preparing and printing the existing tables, which are so discordant among themselves, the necessity of a further investigation becomes very apparent.

The Condensing Hygrometer.

One instrument of this class, that of Mr. Dines, is very simple in construction and not very expensive. In this, cold water or melting ice is employed as a cooling agent, the dew being deposited on a plate of black glass, underneath which the cooling liquid passes, the temperature being indicated by a thermometer near the plate. There are serious objections to this instrument for accurate work. The presence of so much water in the neighborhood of the plate vitiates the result; with a properly arranged Alluard apparatus, shortly to be described, the harmful effects from an open bottle of water on the same table may be easily recognized. Moreover, this instrument cannot be used at temperatures of the dew point below 35°. There is little doubt but that a properly ventilated psychrometer will give much more accurate results for moisture contents of the air than can be hoped for with this instrument. Another instrument of this class is the Alluard form of Regnault's hygrometer. In this the usual cylindrical silver cup gives place to a gilded prismatic box. The usual tubes, one running to the bottom and the other from the top, are supplemented by a third running to the top and terminating on the outside in a little funnel by which the box may be conveniently filled with the cooling fluid. Of the latter, both ether and rhigolene were tried aud preference given to the rhigolene. This may be evaporated more readily, the residuum left after evaporating a large quantity is by no means as great or as harmful in stopping farther evaporation as is the case with ether; finally, it has no affinity for moisture. This latter point is important; in one instance, an operator with ether continu-

ally complained that his fluid congealed at low temperatures; in this case it was not the ether at all, but the moisture taken up by it, that produced the effect. In using rhigolene it is only necessary to empty the box after evaporating a half pint or so; no objectionable deposit is left in the box. The thermometer is held in position by a cork at the top; the cork should be so bored that the thermometer may be brought very near the plate. This plate on which the dew is deposited is brass, plated with gold, and has many advantages over the earlier cylinder of silver. Dew is much more easily detected, the plate is easier to clean, and fumes of the evaporating liquid do not affect the appearance of dew. The writer found the latter an important matter; it was impossible to keep the silver in a satisfactory condition for a large number of observations. To guard against fumes from the evaporated liquid, if in a room, the outlet pipe should be carried to the outside air, and in the open air, the same tube should be placed to the leeward of the instrument.

Before entering upon the use of this instrument it is essential to carefully investigate the errors which are likely to occur. The effect of accidental errors may be practically eliminated by multiplying the observations, but there is a class of constant errors which cannot be thus treated. These may be divided into two classes: 1st, those that may best be investigated by experiments upon a single instrument; and 2d, those which can be completely determined only by the use of two or more instruments. Under the first class the following questions need an answer.

Question 1. What is the effect of using different depths of liquid?

Answer. It is only necessary that the instrument be completely immersed. Experiments in which the instrument was first pushed to the bottom of the box and then pulled to the top of the liquid showed no appreciable difference.

Q. 2. What should be the position of the inlet tube as regards the thermometer and the plate when dew is observed?

A. The inlet tube should be carefully arranged at the back of the box, leaving as much space as possible between it and the thermometer.

Q. 3. What should be the position of the thermometer?

A. It should be as near the plate as possible.

Q. 4. Does the rapidity of flow of air into the liquid need attention?

To answer this question, which has also been regarded as an important one by others, a test tube was arranged with two thermometers placed on opposite sides and as far apart as possible, the inlet tube being passed down very near one of these.

On evaporating the liquid very slowly it was seen that the bubbles arose very near the thermometer that was close by, and there was at first a uniform indication by this of ·3° *lower* temperature than by the other. As the liquid lost its temperature and became much cooler than the air it was found necessary to force in the air much more rapidly in order to continue the cooling process; after a time the two thermometers indicated the same temperature and then the one nearer the inlet tube began to read higher than the other and so continued throughout the experiment, reading half a degree higher for some time. The explanation seems very simple; at first the slowly flowing air only agitated the liquid near its thermometer and hence it caused a lower reading, afterward the air relatively much warmer, flowing more rapidly through the liquid was not entirely cooled to its temperature and hence caused a too high reading of the near thermometer. We may conclude then, that, under both slow and rapid air motion, the thermometer near the plate will not be affected very differently from the plate itself.

Under the second division we seek for answers to the following questions. 1st. What effect does the thickness and material of the plate have? 2d. Are there any appreciable differences in the hygrometric properties of plates? 3d. Is there any difference in the ease of detection of the dew, etc.? I have not had access to apparatus by which to settle these questions, but they may be partly answered by using a single instrument. As quoted above from Mr. Strachan, the thickness of the plate would seem the most important source of error. The following partial answer to the objection is given. It does not seem probable that the *inside* of the plate would have precisely the temperature of the thermometer, but theory would indicate that there would be a gradual diminution of temperature from the outside of the plate to a point inside the liquid, and this would be greater the lower the temperature of the air, as compared with that of the dew point. For testing this question we have the following methods. 1st. We may compare results obtained with a small difference between the air and the dew-point, at which time there would be slight diminution of the temperature of the liquid below that of the front of the plate, with results obtained under just the opposite conditions and which ought to indicate the greater diminution. Such comparison shows an appreciable difference, but this is in precisely the opposite direction from that indicated by theory, i. e., with a small difference between the dew-point and the air, the former is lower, as compared with the air, than with a large difference.

2d. We may lower the temperature rapidly at one time and slowly at another; if we have the same result in both cases we can assume that the effect of the thickness of the plate is inap-

preciable. It is easy to see this, suppose the temperature of the liquid be lowered ·1° per second, and there is a large retardation in the diminution of the outside temperature, or, which is the same thing, the outside temperature is higher because of the air being 40° above the liquid; in such case the thermometer, we will say, will have a temperature ·1° lower than the dew-point, or in other words it takes 1 second for the temperature of the liquid to reach the outside of the plate. Now if we lower the temperature at the rate of ·05° per second then the inside temperature will be ·05° lower than the outside approximately. If now we consider the dew-point constant during the experiment, we shall have with the rapid fall an indicated dew-point ·05° *lower* than with the other. A long series of trials have shown the difference in the two methods inappreciable. It should be borne in mind that whatever be the amount of the effect due to the thickness of the plate, the tendency will always be toward a too low dew-point. A method of testing this question which would seem to be free from many of the objections that may be raised against those already proposed is the following, lack of time and funds have prevented carrying out the plan. We may arrange a plate with two or even three different thicknesses of metal. Suppose such a plate have thicknesses for each third of its width of ·01″, ·02″, ·04″ and it be cooled down gradually; we would expect dew first on the thinnest and last on the thickest, and the difference in temperature as the dew appeared on each could be readily noted.

A second source of error may be due to a difference in the hygrometric properties of plates. It is readily seen that if there be such a difference it would be rather a difficult matter to settle which should be taken as the standard, though it would seem that, so far as this one thing is considered, that plate which gave the lowest dew-point would be the best. No experiments have yet been made to settle this point. It would require observations, from two trained men, at the same time. It may be said, however, that this consideration is far outweighed by others relating to the cleaning of the plate, detection of dew, etc. It is a very difficult matter to clean the plate properly; the best method would seem to be by using a mixture of dilute ammonia and rouge as a wash, applying with silk, thoroughly drying and wiping it off by rubbing; this process gives a most beautiful surface for the dew. Quickness of detection of dew can only be obtained by practice. No fixed rules can be laid down for arranging the light, the position of the eye, etc. It is necessary to cut off all reflections from light-colored objects; this may best be done by placing a black cloth in front of the apparatus. A light breeze is an ad-

vantage in the detection of dew, and in still air this should be raised by a fan. If there is a high wind it will be necessary to put the apparatus in a box, having one of its vertical sides removed, this open side should be placed to the leeward so that the wind will be cut off from the plate.

When in good condition this apparatus works very nicely and seems to follow fluctuations in the dew-point very closely. The following are taken from a number of records illustrating this point.

Fluctuations of the dew-point.

Deerfield, Mass., Nov. 14, 1884.	Mar. 25, 1885.			Oakland, Md.,	Mar. 26.
21·0°	12·0°	11·1°	12·5°	22·6°	19·0°
20·0	11·6	11·6	12·2	22·7	19·4
19·4	11·4	12·3	10·4	23·0	20·0
19·6	10·0	11·4	10·4	23·6	20·7
19·2	10·2	11·2	10·5	23·3	21·1
18·3	10·4	10·9	11·0	23·3	21·6
19·0	9·4	9·0	11·4	24·7	22·2
19·0	10·0	8·7	11·5	25·1	21·2
19·6	9·8	9·7	11·6	23·6	20·2
19·7	9·1	9·7	11·8	23·6	
20·4	9·6	11·0		22·6	
20·7	10·3	10·9			
20·8	11·1				
	11·0				

These dew-points were determined one after the other as rapidly as possible, the interval being not more than 15 seconds between each reading. There seems to be an uniform change in these readings, sometimes the dew-point increases and then diminishes; as all the conditions were constant during each set, it would seem that we have represented an actual fluctuation in the moisture contents of the air.

Still another form of this apparatus is that devised by Crova. The essential characteristic of this is the deposition of dew on the inside of a hollow, horizontal cylinder, the outside of which is cooled in the usual way. The dew is detected by looking through an eye-piece at one end of the cylinder, and the appearance is said to be highly satisfactory. A most serious objection to this form is the extreme difficulty of properly cleaning and polishing the inside of the cylinder; another is the impossibility of attaining very accurately the temperature of the surface on which the deposition takes place. Some remarkable results have been obtained with this apparatus and have been published in the Zeitschrift für Meteorologie, 1884, p. 46. In the following table there are given: 1st, the result out of each set of three which showed the greatest difference between Reg-

nault and Crova; 2d, the mean of each set of three, and the mean of all three sets. The value of A, in the psychrometric formula (soon to be described) $x=f-A(t-t')\frac{5}{9}h$, has been assumed as ·00080 for the observations with the Regnault apparatus and the corresponding value with the Crova instrument has been computed.

Comparison of Regnault and Crova.

No. of Obs.	Dry observed	Wet computed	Dew-point observed. Regn.	Crova.	Value of A. Regn.	Crova.
	°	°	°	°		
1	72·5	58·0	44·1	46·9	·00080	·00067
1	67·5	56·8	46·9	49·6	·00080	·00067
1	77·9	65·4	56·7	59·2	·00080	·00059
6	70·7	56·7	42·7	44·2	·00080	·00073
4	67·0	57·0	48·0	49·2	·00080	·00073
6	78·2	65·5	57·0	59·1	·00080	·00063
16	72·7	60·0	49·4	51·2	·00080	·00069

These results are extraordinary, and are probably due in large measure to an improper condition of instruments rather than to an actual difference in their normal indications. Certainly differences greater than ·5° F. ought not to occur in a properly observed Regnault apparatus, while these comparisons show an extreme range in the differences between the two instruments of 2°·9. I have dwelt at length on these results as they show clearly some of the difficulties to be met with in comparisons of this kind. I add a remarkable comment by a prominent writer, on the original observations. "In the observations given by Crova we find, as was to be expected, that Regnault's apparatus gives dew-points too low by as much as 1°·6 C."

The Psychrometer.

As is well known, this consists of two thermometers; one of them, the wet, has a muslin over the bulb, which is kept continually moistened by a wick attached above the bulb and dipping in a cup of water, and measures the temperature of evaporation; the other is dry and indicates the air temperature. The amount of moisture makes but little difference; Regnault found that if water dripped slowly from the bulb, he still obtained accordant results; this, however, was in still air. Since the wet bulb may at times stand 20° below the dry, it might be supposed that the stem of the thermometer immediately above the bulb would tend to raise the temperature. This effect is very slight, amounting in the case of a spherical bulb to ·3° when the air is perfectly still. The muslin should be very thin and may be made to fit a cylindrical bulb very tightly, if a piece the right size is first immersed in water and then put on the bulb, a string being tied upon it just above

and below the bulb. The water used should be rainwater, if possible. In the winter season, or when the wet bulb temperature is below freezing, great care must be taken in coating the bulb with a uniform layer of ice. To do this properly, it is necessary, whenever it is found that the air temperature is going below freezing, to take the wick out of the cup and hang the free end over the support behind; it should not be taken off the thermometer. If this wick becomes stiffened with ice before it has been taken out of the cup and cannot be bent, the whole, including bulb, cup and wick, must be immersed in luke-warm water, lifted up from beneath, till the wick is sufficiently pliable to permit properly adjusting as above. To wet the bulb, keep the cup of water as near 32° as possible and 10 to 25 minutes before the observation, if there is no artificial ventilation, lift the cup up from beneath and wholly immerse the bulb; in withdrawing the cup, wait a moment after the bulb is exposed and touch the drop that will be hanging from it, with the edge of the cup. If there is a high wind, two or three coats at once may be needed to obtain a good result. A mass of ice should not be allowed to accumulate on the stem or about the bulb, but it should be melted off and a uniform coat applied. No brass scale or protector should ever be allowed within 1·5 inches of the wet bulb. This scale has the temperature of the air, and if the muslin or wick touch the scale its temperature will be conducted to the bulb, thus giving too high a reading. If the muslin becomes brown or covered with dust it should be replaced by clean; this may need to be done once a month in some places. Frequently the muslin will be found rather firmly fastened to the bulb by the formation of a lime-like substance; in such case, the bulb may be cleaned by using a bath of dilute muriatic acid.

When the temperature reaches about 20° there seems to be a peculiar effect produced which causes the indicated temperature to be too high and which increases as the temperature falls. This may be due to a contraction of the ice-film or coating; at 0° the effect amounts to nearly 5°, but after that point is reached the ice seems to partly give way, as the contraction seldom goes beyond 5°. Experiments have been made by immersing dry and ice-covered bulb thermometers in a bath of mercury and also in disulphide of carbon with identical results. As long as the temperature was lowered or remained stationary, even down to −20°, there was still a marked effect observed, but the moment the temperature began rising, possibly because the ice expanded so much more rapidly than the glass, the two thermometers began to approach each other and quickly agreed. The above explanation seems extraordinary

and it may be necessary to seek another. It may be that at low temperatures the ice refuses to conduct the very last part of the heat from the bulb; however, this explanation does not answer when the temperature begins rising, as in that case the ice-covered bulb ought to again lag and be slightly cooler than the liquid.

The writer's attention was first called to this matter by the astonishingly high relative humidities reported from the northwestern stations, whenever a high-pressure area with very low temperatures appeared. At such times the relative humidity is almost invariably reported at 100 per cent, and yet ordinarily these same high areas, with N.W. winds, frequently reduce the humidity 50 and even 60 per cent when they reach places toward the S.E. where the temperature is above 20°. Again, many observers have reported the ice-covered bulb reading higher than the dry on cold days, even when they had corrected thermometers and very high winds: it was thought that many times the trouble arose from a lack of ventilation and an improper coating of ice. Later experiments, however, with perfectly correct thermometers, most careful ice-coats, good ventilation and a nearly saturated air at temperatures down to 0° have repeatedly confirmed these abnormal results, showing in one instance the iced bulb ·5° higher than the dry. When we consider that at −20° a difference of ·5° between dry and wet amounts to 36 per cent in the deduced relative humidity, the great importance of a thorough investigation of this point becomes very apparent. We must know the amount of this contraction with different coatings of ice and at different temperatures; knowing these facts, we may apply a correction to the wet bulb-reading before using it for computing the moisture content of the air. The best method of testing the question would seem to be by a rigid series of comparisons at very low temperatures between the ice-covered bulb and a condensing hygrometer.

In the use of the psychrometer for making the best observations the most important consideration is that of a thorough ventilation, and this is especially to be attended to if readings are to be made in a room or very still air. Out of doors, if there is a good air circulation about the wet bulb, artificial ventilation is not needed in the summer, but with an ice-covered bulb the action is so sluggish that it is very convenient to have a movable fan for increasing the air circulation and expediting the observation. The most convenient method of ventilating the wet bulb is by swinging in a circle of some two feet radius. This method seems to have been first adopted by Saussure at the end of the last century; he employed a swung wet bulb for obtaining the temperature of evaporation but did not join with

it a thermometer for obtaining the air temperature at the same time, the latter being an important consideration. Most careful experiments have been made looking to the determination of all the errors to which this sling psychrometer may be supposed to be subject. At the greatest velocities attainable by hand, no harmful effects have ever been noted from friction with the air or from expansion of the bulb through the centrifugal action of the mercury column. Any convenient velocity may be used; about four meters per second, ten miles per hour, was found satisfactory. The results were precisely similar at all velocities from two to ten meters per second, great care being taken in wetting the bulb uniformly during the entire time of swinging, and also continually changing the plane in which the bulb was swung. Repeated trials in a room have shown that an accuracy of a tenth of a degree may readily be attained with this device in one minute. In the open air it is necessary to swing out of sunshine and in a moderate shade only, if the air temperature is desired. It has been thought by some that at night with a clear sky, radiation from the bulbs into space is harmful, but experiment shows that the amount of this radiation from bulbs suspended in a still air is very slight, and the rapid motion entirely annuls any evil effect. For the best results it is essential that the sling wet bulb should be continuously moistened. To do this properly requires some skill and constant watchfulness. The muslin should be carried up the thermometer stem about an inch, and to the latter there should be fastened a tube of glass drawn out to a point. The point should be placed just at the top of the muslin, or at least one inch from the bulb, otherwise its heat imparted to the water will tend to raise the temperature. A string inserted in this tube may be pressed down or pulled out according as the air is moist or dry, the intention being to keep the muslin constantly and uniformly moistened.

Comparisons.

The investigations of others have established a formula for comparing these instruments, which may be taken as a working hypothesis, as follows, for temperatures above freezing:

$x = f - \frac{5}{9}(t - t')\,\mathrm{A}h$, in which
x= the tension of vapor of saturated air at the dew-point temp.
f= the tension of vapor of saturated air at the wet-bulb temp.
t= air temperature.
h= air pressure in inches of mercury.
A= a constant to be determined.

For temperatures below 32°, it has been thought by some even to the present time, from theoretical considerations, that

the evaporation from ice is different from that from water, and hence the value of A must change suddenly at 32°. If there be such a change allowed for, the distinction should be between water and ice and not above or below 32°, for frequently the wet bulb may be lowered to below 26° without the formation of ice. This is an important problem, and has been practically solved in various ways. In March, 1884, the writer, with the assistance of Mr. Fassig, performed some experiments in a room with the temperature just below freezing. It was found possible to have two wet bulbs in operation at the same time as sling thermometers, the one being covered with water and the other with ice. The difference between the two was practically inappreciable The results in detail may be found in the American Meteorological Journal for June, 1884. Later comparisons between a condensing hygrometer, water- and ice-covered bulbs confirmed the above conclusion. The results of these may be found in the journal just quoted, for February, 1885. Even before that time somewhat the same conclusion had been arrived at in Europe, though the experiments here were entirely independent of those.

Value of A.

Having given the dew-point, t, t' and h, we can obtain the constant of the formula. As t' enters the formula twice, it has by far the greatest influence and it is partly because it has been found impossible to obtain an invariable reading of t' that so many differing values of A, have been obtained; a few examples will suffice.

In still air Regnault found A= ·00128; in the open air ·00074: Doyere with the sling psychrometer in 1855 from a very few observations in hot weather found ·000687; Sworykin, with fair ventilation ·000725; Blanford ·000827; Angot from over 3,000 observations ·000851; Chistoni ·000851. The latter three results are probably without any uniform ventilation. The first comparisons by the writer were made in Feb. 1883, with a bellows for a ventilator of the dry and wet bulbs; these results were published in Science, June 8, 1883. A longer series of experiments were instituted in October, 1884. From 700 sets of observations, both indoors and out, with high and low temperatures, and with large and small differences between t and t', a value of A was obtained at ·00068 and upon this value was computed the table for relative humidity to be found at the end of this paper. The value of A that has been adopted may seem rather small, nevertheless, it agrees exactly with that adopted by Regnault for temperatures below freezing, so that no change is made in the table below that point. This value depends upon the separate determination of three quantities,

each of which has usually had great uncertainties entering into it. It is believed that the errors entering into t and t' have been reduced for each individual observation to less than ·1°. It is surprising to see how swing after swing may be made with the sling psychrometer resulting in absolutely no change in the readings. The same cannot be said, however, regarding the determination of the dew-point; in this errors will creep in on different days of ·3°, and in consequence the value of A may be changed slightly. When there is a small difference between t and t' there is great difficulty in getting uniform results, but when $(t-t')$ is small a larger difference from the mean in A produces a much smaller effect upon the deduced moisture content than when $(t-t')$ is large. With $(t-t')$ equal to or greater than 15° there is no difficulty in getting A within two of the fifth place again and again and on different days. The vapor tensions employed in computing the table have been taken as recently computed by Broche from Regnault's observations.

One of the more interesting questions which arises in connection with the formula is the effect of hight above sea upon the relation between the $dp:t$, and t'. Glaisher, after bringing together many thousands of comparisons between a Daniel's hygrometer and the psychrometer, at various hights in India and elsewhere, decided that hight had no appreciable effect upon the deductions to be made with the psychrometer. Although these observations were made without any uniform ventilation for the psychrometer and cannot be regarded as giving absolute results at either sea level or on mountain tops, yet it would seem as though they were of sufficient accuracy to enable relative comparisons so far as the question of hight comes in, i. e., if the same instruments observed in precisely the same way at two hights give the same results, we may infer that there is no difference in the relations of the quantities measured, although large errors may occur at both hights. The question of a possibly better ventilation at the higher station need not be considered, as this would have tended to lower the wet-bulb reading and would have been in exactly the direction of theory at high stations. The exposures and amount of moisture at the two hights would have a slight influence, but in a long series of observations there would be an elimination of these effects. Dr. Pernter has published a series of observations with a ventilated psychrometer,* and Regnault's condensing hygrometer at a barometric pressure of about 600^{mm}. These observations gave a mean value of A as ·001042; while the value of A at sea level adopted by Dr. Pernter is ·000843. If we multiply

* This psychrometer was in a ventilated screen of Prof. Wild's form, but Prof. Wild thinks that the screen used was his earlier form with a clock-work ventilator, which did not give a very perfect ventilation.

the first of these by 600 and the second by 750, the approximate air pressure at the two hights, we obtain ·625 and ·632 respectively, showing that Ah was nearly constant and that the relations to each other of the other three quantities are practically the same at all altitudes. These observations, however, were not sufficiently numerous and were not made with a psychrometer well enough ventilated to give satisfactory results.

In order to test the question a week was spent, in March, 1885, at Oakland, Md., at a hight of 2,800 feet above sea level and where the pressure was about 27″. A series of comparisons at this point made with the identical instruments and under precisely the same conditions as those previously made at sea level gave a value for Ah of ·0197, while the sea level value was ·0204. If we assume h as 30″ in both these cases, we shall have A ·000657 and ·000680. The temperatures employed at Oakland ranged from 102° to 14°, and with the former the dewpoint was 48° lower than the air. These observations were not sufficiently extended to give an absolute value for Ah, but the relative comparisons may be regarded as approximately accurate. Any one acquainted with the use of these instruments will recognize the very small margin there is in the two values above. The difference in the computed humidity from either of the values will be nearly inappreciable. Moreover, whatever may be the absolute relation between the $dp : t$ and t' under different pressures, none but the most refined observations can by any possibility detect it, and so far as ordinary observations are concerned no account need be taken of this difference up to 3,000 feet. This is especially apparent when we consider the extreme difficulty under the most satisfactory conditions of obtaining even an approximate value of the wet and dry temperatures, and it should be borne in mind that these difficulties are not due, for the most part, to accidental errors which may be averaged out by multiplying the observations, but are caused by constant errors, such as imperfect ventilation, too high temperature through the day, and too low just after sunrise for an hour or two, etc. The question of applying a correction for elevation to the indications of a psychrometer depends largely upon the practical benefit to be derived. No such correction is needed in order to make intercomparisons at high stations, but it may be argued that it is needed in order to obtain, first, a comparison between a high station and a neighboring lower one, and second, to obtain the diminution in moisture contents at any time or during any season between sea level and the upper atmosphere that is passing a high station. To these arguments it may be answered, first, we do not know the variations of humidity in different strata so as to obtain a law of reduction

to sea level and make a comparison at all possible, and second, the conditions of moisture are so variable on mountain tops, so influenced by local surroundings and by currents of air laden with moisture condensed by the mountain side, so utterly different from the conditions in the free air strata at the same hight, that no comparisons of any practical value can be made. These considerations tend to show that the use of a special table for stations up to 3,000 feet altitude is very questionable, and in the present stage of meteorology a refinement entirely unnecessary, especially for individual observations. The additional labor and watchfulness needed in examining all records to see whether the table has been properly applied, and above all, the extreme annoyance resulting from an attempt at interpolating for hight in a table, and the great danger of making errors in such interpolation, far outweighs any theoretical advantage to be gained, which, as has already been shown, is in itself very questionable. While the table accompanying this paper is applicable to the sling psychrometer, yet most careful comparisons between the sling and a psychrometer in a fairly open shelter having a good exposure, have shown little or no difference; this may easily be understood, since velocities of wind as low as two meters per second have given as good results as those five times as great. The table is applicable to all open shelters except those from windows and on walls; in such cases there should be used, if possible, some means for artificial ventilation. For all traveling parties and at all stations where the temperature is often below freezing, the sling psychrometer is on all accounts the best instrument for obtaining air humidity.

The experiments for this paper were completed last spring, but it has been found impossible to study the great mass of data and properly prepare it for publication before this time.

October 12, 1885.

H. A. Hazen—Table of Relative Humidity.

$$x = f - .00068\,(t-t') \times \tfrac{5}{9} \times 30 \quad \text{R. H.} = \frac{x}{F}.$$

x = vapor tension at dew-point temperature.
f = " " wet bulb " or t'.
F = " " air " or t.

$t-t'$

t	0·0	1·0	2·0	3·0	4·0	5·0	6·0	7·0	8·0	9·0	10·0	11·0	12·0	13·0	14·0	15·0	16·0	17·0	18·0	19·0
0	100	70	41	11																
1	100	71	43	15																
2	100	72	45	18																
3	100	73	47	21																
4	100	74	49	24																
5	100	75	51	27	3															
6	100	76	53	30	7															
7	100	77	55	33	10															
8	100	78	56	35	13															
9	100	79	58	37	17															
10	100	80	59	39	20															
11	100	80	61	41	22	3														
12	100	81	62	43	25	7														
13	100	82	63	45	28	10														
14	100	82	65	47	30	13														
15	100	83	66	49	33	16														
16	100	84	67	51	35	19	3													
17	100	84	68	53	37	22	6													
18	100	85	69	54	39	24	10													
19	100	85	70	56	41	27	13													
20	100	86	71	57	43	29	15	1												
21	100	86	72	58	45	31	18	4												
22	100	86	73	59	46	33	20	7												
23	100	87	74	61	48	35	23	10												
24	100	87	74	62	49	37	25	13	1											
25	100	88	75	63	51	39	27	16	4											
26	100	88	76	64	52	41	29	18	7											
27	100	88	77	65	54	43	31	20	10											
28	100	89	77	66	55	44	33	23	12	1										
29	100	89	78	67	56	46	35	25	15	4										
30	100	89	78	68	58	47	37	27	17	7										
31	100	89	79	69	59	49	39	29	19	10										
32	100	90	80	70	60	50	40	31	22	12	3									
33	100	90	80	70	61	51	42	33	24	15	6									
34	100	90	81	71	62	53	43	34	26	17	8									
35	100	90	81	72	63	54	45	36	28	19	11	2								
36	100	91	82	73	64	55	46	38	30	21	13	5								
37	100	91	82	73	65	56	48	39	31	23	15	7								
38	100	91	83	74	66	57	49	41	33	25	17	10	1							
39	100	91	83	75	66	58	50	42	35	27	19	12	4							
40	100	92	83	75	67	59	51	44	36	29	21	14	7							
41	100	92	84	76	68	60	52	45	38	30	23	16	9							
42	100	92	84	76	69	61	54	46	39	32	25	18	11	2						
43	100	92	85	77	69	62	55	47	40	34	27	20	13	5						
44	100	92	85	77	70	63	56	49	42	35	28	22	15	8						
45	100	93	85	78	71	64	57	50	43	36	30	24	17	11	5					
46	100	93	86	78	71	64	58	51	44	38	31	25	19	13	7	1				
47	100	93	86	79	72	65	59	52	45	39	33	27	21	15	9	3				
48	100	93	86	79	72	66	59	53	47	40	34	28	22	16	11	5				
49	100	93	86	79	73	66	60	54	48	42	36	30	24	18	12	6	1			
50	100	93	86	80	73	67	61	55	49	43	37	31	26	20	14	8	3			

H. A. Hazen—Table of Relative Humidity—continued.

	$t-t'$																		
t	*0·0*	*1·0*	*2·0*	*3·0*	*4·0*	*5·0*	*6·0*	*7·0*	*8·0*	*9·0*	*10·0*	*11·0*	*12·0*	*13·0*	*14·0*	*15·0*	*16·0*	*17·0*	*18·0*	*19·0*
50	100	93	86	80	73	67	61	55	49	43	37	31	26	20	14	8	3			
51	100	93	87	80	74	68	62	56	50	44	38	32	27	22	16	10	5			
52	100	93	87	81	74	68	62	56	51	45	39	34	28	23	18	12	6	1		
53	100	94	87	81	75	69	63	57	52	46	41	35	30	25	19	13	8	3		
54	100	94	87	81	75	69	64	58	53	47	42	36	31	26	21	15	10	5		
55	100	94	88	82	76	70	64	59	53	48	43	38	32	27	22	17	11	7	2	
56	100	94	88	82	76	70	65	60	54	49	44	39	34	29	24	18	13	9	4	
57	100	94	88	82	77	71	66	61	55	50	45	40	35	30	25	20	15	10	6	1
58	100	94	88	83	77	71	66	61	56	51	46	41	36	31	27	22	17	12	8	3
59	100	94	88	83	78	72	67	62	57	52	47	42	37	33	28	23	18	14	10	6
60	100	94	89	83	78	72	67	62	57	52	48	43	38	34	29	25	20	16	12	8
61	100	94	89	83	78	73	68	63	58	53	48	44	39	35	30	26	22	17	13	9
62	100	94	89	84	79	73	68	64	59	54	49	45	40	36	31	27	23	19	15	11
63	100	95	89	84	79	74	69	64	59	55	50	46	41	37	33	28	24	20	16	12
64	100	95	89	84	79	74	69	65	60	55	51	47	42	38	34	30	26	22	18	14
65	100	95	90	84	80	75	70	65	61	56	52	47	43	39	35	31	27	23	19	15
66	100	95	90	85	80	75	70	66	61	57	52	48	44	40	36	32	28	24	20	17
67	100	95	90	85	80	75	71	66	62	57	53	49	45	41	37	33	29	25	21	18
68	100	95	90	85	80	76	71	67	62	58	54	49	46	42	38	34	30	26	23	19
69	100	95	90	85	81	76	72	67	63	59	54	50	46	42	39	35	31	27	24	20
70	100	95	90	86	81	76	72	68	63	59	55	51	47	43	39	36	32	28	25	22
71	100	95	90	86	81	77	72	68	64	60	56	52	48	44	40	37	33	29	26	23
72	100	95	91	86	81	77	73	68	64	60	56	52	49	45	41	38	34	30	27	24
73	100	95	91	86	82	77	73	69	65	61	57	53	49	46	42	39	35	31	28	25
74	100	95	91	86	82	78	73	69	65	61	57	54	50	46	43	39	36	32	29	26
75	100	95	91	86	82	78	74	70	66	62	58	54	51	47	43	40	37	33	30	27
76	100	95	91	87	82	78	74	70	66	62	58	55	51	48	44	41	37	34	31	28
77	100	96	91	87	83	79	74	70	67	63	59	55	52	48	45	42	38	35	32	29
78	100	96	91	87	83	79	75	71	67	63	59	56	52	49	46	42	39	36	33	30
79	100	96	91	87	83	79	75	71	67	64	60	56	53	50	46	43	40	37	34	30
80	100	96	91	87	83	79	75	71	68	64	60	57	54	50	47	44	40	37	34	31
81	100	96	91	87	83	79	76	72	68	64	61	57	54	51	47	44	41	38	35	32
82	100	96	92	87	84	80	76	72	68	65	61	58	55	51	48	45	42	39	36	33
83	100	96	92	88	84	80	76	72	69	65	62	58	55	52	49	46	43	39	37	34
84	100	96	92	88	84	80	76	73	69	66	62	59	56	52	49	46	43	40	37	35
85	100	96	92	88	84	80	77	73	69	66	63	59	56	53	50	47	44	41	38	35
86	100	96	92	88	84	80	77	73	70	66	63	60	57	53	50	47	44	41	39	36
87	100	96	92	88	84	81	77	74	70	67	63	60	57	54	51	48	45	42	39	36
88	100	96	92	88	85	81	77	74	70	67	64	61	57	54	51	48	46	43	40	37
89	100	96	92	89	85	81	78	74	71	67	64	61	58	55	52	49	46	43	40	38
90	100	96	92	89	85	81	78	74	71	68	64	61	58	55	52	49	47	44	41	38
91	100	96	92	89	85	81	78	75	71	68	65	62	59	56	53	50	47	44	42	39
92	100	96	92	89	85	82	78	75	72	68	65	62	59	56	53	50	48	45	42	40
93	100	96	92	89	85	82	78	75	72	69	65	62	59	56	54	51	48	45	43	40
94	100	96	93	89	85	82	79	75	72	69	66	63	60	57	54	51	49	46	43	41
95	100	96	93	89	85	82	79	76	72	69	66	63	60	57	55	52	49	46	44	41
96	100	96	93	89	86	82	79	76	73	69	66	63	61	58	55	52	50	47	44	42
97	100	96	93	89	86	82	79	76	73	70	67	64	61	58	55	53	50	47	45	42
98	100	96	93	89	86	83	79	76	73	70	67	64	61	58	56	53	50	48	45	43
99	100	96	93	90	86	83	80	76	73	70	67	64	62	59	56	53	51	48	46	43
100	100	96	93	90	86	83	80	76	74	71	68	65	62	59	56	54	51	49	46	44

ART. LV.—*A new form of Absorption Cell;* by ARTHUR E. BOSTWICK.

THE writer has devised and used the cell described below for the purpose of obtaining the absorption spectra of liquids which have but little selective absorption, and which would therefore have to be used ordinarily in large quantities.

The cell is a rectangular box about six inches long by three broad and three in height. The bottom and the two ends are of pine wood, covered with shellac, and the two sides are of ordinary looking-glass, cemented to the wood, so that the box is water-tight. The reflecting surface of the looking-glass is turned inward and at each of two diagonally opposite corners the amalgam is scraped away so as to make a vertical slit about two millimeters in width. One of these is placed close to the spectroscope slit, and through the other a parallel beam of light is admitted. It is evident that the box may be so placed that the beam will be internally reflected in it a number of times, depending upon the angle between the two, and will finally pass through the second slit into the spectroscope. The length of its path through the cell may therefore be varied indefinitely by turning the latter, and is limited only by the decrease in intensity caused by general absorption—not only in the liquid, but also at each reflection.

A solution of bichromate of potash, so weak that a test-tube full of it was of a barely perceptible yellow color and showed no absorption at all when held before the spectroscope slit, when placed in this cell, absorbed the whole upper end of the spectrum, the F line being scarcely visible. In this case sunlight was used, the beam being reflected six times, and having a path whose length inside the cell was about two feet. With mirrors of polished metal the result might be even better, since the absorption in the glass would be eliminated. In this case however the number of liquids which could be used in the cell would be somewhat limited.

ART. LVI.—*Preliminary notice of Fossils in the Hudson River Slates of the Southern part of Orange Co., N. Y., and elsewhere;* by NELSON H. DARTON.

IN a very detailed study, now nearly completed, of the formations other than Archæan in central and eastern Orange Co., N. Y., fossils have been discovered in many new localities

* This Journal, Sept., 1880, pp. 197, 198.

in several formations which have thrown much light upon the complicated stratigraphical structure of the district. An account of the results of the investigation will soon be ready for publication.

The principal paleontological discoveries are in continuation of those of Professor Dwight and Mr. Dale nearer Newburg, and the one to which attention is now called is the finding of Trenton and Hudson River fossils in the Hudson River (?) slates at several widely distant points.

The first and most important is near Sugar Loaf village, twenty-one miles S.W. of Newburg, also at Walden on the banks of the Wallkill, eleven miles N.W. of Newburg, and at an intermediate point, Rock Tavern, ten miles west of Newburg. Two other probable localities are known, but have not as yet been sufficiently explored for description.

Mather, in his final Report on the First District, 1843, p. 369, states, in referring to the Testacea of the Hudson River Group, "a few were observed . . . . . near the villages of Walden and Sugar Loaf," but so far as I can determine this statement stands alone until now, when I can confirm and augment it.

Near Sugar Loaf village most of the fossiliferous beds lie to the west of the station, and at Bulmer's Quarry a bed was discovered that yielded all the species found. Its thickness was about 40 mm.; it was composed almost entirely of fossils and fragments mixed with a soft red earthy matter and not crossed by the very prominent cleavage planes of the enclosing slates; other thinner beds of this material were found and a few indeterminable Crinoidal stems were observed in the rock on the more or less obscure bedding planes; no fucoid remains were recognized. The fossils were more or less decomposed and often contorted, but many specimens were beautifully distinct. The following remains were identified and are given in the order of their abundance.

Orthis pectinella H.; *O. testudinaria; O. plicatella* H.; *Leptæna sericea* Sow.; *Camerella hemiplicata* H.; *Strophomena alternata* Cv.; *Streptorhynchus planimbona* or *S. felifexta?* H.; and a *Trinucleus concentrica* Eaton, was recognized by a portion of the border of a cephalic shield. Several fragments were found which showed no specific character, especially a *Chætetes*, a *Favosite*, and crinoidal columns.

Occasional beds bearing more or less distinct fossils were traced eastward from the above locality for several thousand feet, nearly to the Archæan rocks of Sugar Loaf hill and dipping under them with a steep S.E. dip.

At Rock Tavern, fossils were discovered in the shales exposed in the new railroad cut at points where alteration was

less, and the bedding planes more distinct. The remains found were of *Leptæna sericea* and *Orthis testudinaria* in about equal proportions, generally greatly contorted and in various stages of distinctness. The slates dip N. 80° W. >40°, and grade insensibly above and below into highly altered strata contorted and seamed in every direction, especially the underlying beds. This disturbed area is a local one, however, and much progress has been made in this district in tracing out the foldings of the slates. This fossiliferous bed is not far from the base of the formation, and is not intercalated in the other members by folding in an overturned synclinal.

The fossils found near Walden were at the junction of beds of grauwacke and thin layers of fossil slates, exposed in the quarries below the bridge and elsewhere. *Leptæna sericea* was quite abundant at one point, and *Orthis testudinaria* was associated but in less numbers, also *O. pectinella* and a fragment of *Conularia* probably *Trentonensis*. The fossils were finely preserved and the subjacent rock contained numerous small nodules of soft, bituminous matter as noted by Horton in his Report to Professor Mather in 1839.* They are often 10 mm. in diameter. Several indistinct fucoid impressions appear on some of the layers but none were recognizable.

The rocks dip southwestward at a moderate angle, and stratigraphic studies in this district make it appear probable that the fossiliferous beds are at a low horizon in the formation. Further details of the stratigraphic structure will be given in the paper before noted.

In conclusion, I wish to acknowledge my indebtedness to Professor R. P. Whitfield for his kindness in determining the greater number of the species for me, and for his aid and advice at many times in the investigation.

ART. LVII.—*Report of the American Committee-delegates to the Berlin International Geological Congress, held Sept. 28th to Oct. 3d*, 1885; by PERSIFOR FRAZER, D.Sc., Secretary.

THE following report of the third session of the International Geological Congress in Berlin, was made from notes taken by the Secretary of the American committee for this session. These notes were afterwards written out in full, with the kind assistance of Professor Williams to whom the writer hereby expresses his sincere obligations. It will be remembered that the inception of this most important gathering was the action of a com-

* 3d Annual Report of Natural History Survey, p. 144.

mittee at the Buffalo meeting of the American Association for the Advancement of Science, in 1876, the year of the Centennial Exposition. This committee left the work of organizing the first Congress in the hands of a committee of geologists, who thereupon selected as the date of the first Congress the year 1878, or that of the French "Exposition Universelle." The first Congress was duly held, and was remarkable for nothing so much as the absence of any representatives of Germany. After settling some preliminary matters it was decided that the next sitting of the Congress should be held at Bologna, in 1881.

This second session of the Congress was also held; and by this time, the methods of accomplishing the ends of unification in nomenclature and coloring having become better understood, it was determined to undertake to make a map of Europe on a scale of $\frac{1}{1,500,000}$. A committee was appointed to take this in hand, and another to devise ways and means of making a consistent nomenclature for the science.

The two committees met at Foix and Zurich during the four years that intervened between the Congress of Bologna and that of Berlin, and the work of the present Congress has been mainly the adoption of the propositions made at these meetings. At the meeting of the American Association for the Advancement of Science at Ann Arbor last summer, Professor H. S. Williams and Professor Persifor Frazer were added to the original committee, constituting the American delegation and actually represented at Berlin by Professors James Hall and J. S. Newberry. Professor Brush, who was in Berlin at the time, was elected by the committee one of the members. The members of the American committee present at the time of the opening session of the Berlin Congress were, Prof. James Hall (President), Prof. J. S. Newberry, Prof. Brush, Prof. H. S. Williams and Prof. Persifor Frazer (Secretary).

The International Geological Congress met at its third session in Berlin, Monday, September 28. The members and delegates arriving before this date registered at the office of the Bureau in the Bergakademie. The council met at 11 o'clock Monday morning to determine upon the programme of the first session and nominate officers of the present meeting, and at 5 o'clock the members of the Congress assembled at the house of the Reichstag for mutual greetings. Only members of the congress were admitted, and those having registered and received the card of membership were presented with the badge of the Congress, which was in the form of a medal, with the well-known geological and mining symbol of crossed hammers in

the center under which are the words "Geologorum conventus —mente et malleo;" and on the reverse—"Berlin, 1885."

The formal opening of the Congress took place Tuesday morning, September 20th, at 11 o'clock, at the House of Deputies.

At this meeting, Professor Capellini, of Italy, occupied the chair as President of the Congress at Bologna.

On his right was Dr. von Dechen and M. Hauchecorne, on the left Professor Beyrich and Professor Hall.

On the ministerial benches on the right were the diplomatic and government officers, and on the left the vice-presidents, representing various countries.

Professor Capellini introduced the "Cultus-Minister," Herr von Gossler, who welcomed the Congress in German.*

Herr von Gossler dwelt upon the fact that no science could proceed in any direction without calling to its aid the assistance of the other sciences. He noted the advantage which had accrued to astromomy by this course. He reminded his hearers that Prussia had been the home of von Buch and von Humboldt, and in the name of the Prussian government he warmly appreciated the honor conferred upon Berlin by its choice as their place of meeting, and bade them welcome with the miner's greeting "Glück auf." He added humorously, that as the facts of geology rest upon the results of the action of water, he knew the present weather would not deter the true geologist from his work.

Dr. von Dechen then read his address in French, beginning with thanks to the members for having elected him honorary President. He called to mind the names of many men of European science of a past generation, specifying among them some of the greatest with whom he had been intimate in Paris, in London and in Germany. He stated that much had been done in Geology since the last Congress at Bologna, and much still remained to be done. After thanking the government for its kind reception of the guests, he concluded by expressing the high appreciation of the people of Berlin of the honor done them by the Congress in meeting in their midst.

Professor Capellini then addressed the Congress. His first words were that he owed the honor of occupying the chair to the fact of his having been chosen to preside over the Congress at Bologna. He sketched the origin and history of the Congress from the time of its inception by the committee of the American Association for the Advancement of Science in 1876, through the Paris sessions in 1878 and the Bologna Congress of 1881, and mentioned particularly the very friendly attitude which his Majesty, the king of Italy, had assumed toward its

* By the action of the Congress at Bologna the language of its debates is French.

work and deliberations. He continued: "I had the honor in the month of August last, to communicate to his Majesty, King Humbert, the project of holding the third session of the Congress in Berlin, and his Majesty specially charged me to convey to the officers and members his kindliest greeting, and the assurance of his sincere interest in the result of its deliberations, and desired me to be the interpreter of his wishes for its complete success. (Hearty applause.) In conclusion I have the honor to announce, that Dr. Beyrich has the floor." Hereupon Dr. Beyrich read from manuscript his address in French. It was an exhaustive history of the development and proceedings of the Congress up to the Bologna session, and also of the successive meetings of the committees on the chart of Europe at Foix and at Zurich. He also gave an account of the meeting of the German committee at Stuttgart in 1883. The meeting of the Congress at Berlin was determined upon for 1884, but was postponed on account of the cholera. The objects of the Congress—the discussion and determination of questions of geological classification, nomenclature and cartography were explained, and a general account of the results already attained was given. He closed by calling attention to the charts and collections in the Bergakademie which illustrated these results.

At the conclusion of the address of Dr. Beyrich, the following list of nominations for officers of the council for the Berlin session was read by Professor Capellini, and the nominations there made were elected by unanimous vote of the Congress, whereupon M. Capellini yielded the chair to Dr. Beyrich.

Members of the Bureau.

Honorary President: Dr. von Dechen.

President: Prof. Beyrich.

Vice-Presidents: Messrs. Credner, Fraas and von Gümbel, of Germany; Stur, of Austria; Dewalque, of Belgium; Johnstrup, of Denmark; Vilanova, of Spain; James Hall, of the United States; Jacquot, of France; Hughes, of Great Britain; Szabó, of Hungary; Blanford, of India; de Zigno, of Italy; Kjerulf, of Norway; van Calker, of Holland; Choffat, of Portugal; Stefanescu, of Roumania; Inostranzeff, of Russia; Torell, of Sweden; Renevier, of Switzerland.

General Secretary: M. Hauchecorne.

Secretaries: Messieurs Fontannes, Bornemann père, Fornasini, Wahnschaffe.

Treasurer: M. Berendt.

Members of the Council: Messrs. Benecke, Dupont, Böckh, Ewald, Frazer, Gaudry, Geikie, Giordano, von Hantken, de Lapparent, Lepsius, Mayer-Eymar, von Mojsisovics, Newberry, Pilar, Platz, Strüver, Topley, Williams, Zittel.

At the opening of the evening session, at 6 o'clock, M. Hauchecorne, the secretary, requested the members to inform the Bureau of any ladies they might have with them, in order that means might be provided for their comfort and entertainment. The first printed list of members was then presented.

The secretary further stated, that catalogues of the museums of science and of arts had been prepared and would be distributed at the close of the meeting. The Prussian minister had provided for the opening of the museums to all members of the Congress, from 9 o'clock till 3 P. M., and certain days were designated when the chiefs, or their representatives, would be present to show and explain their contents.

M. Renevier (of Switzerland), chairman of the committee on *the chart of Europe*, then commenced to read the report of his committee, explaining beforehand that it was not *his* report but the report of the committee which had met at Foix and at Zurich, and deliberated upon the matters referred to them. Although the place and times had been appointed for the discussion of all matters pertaining to the preparation of the geological map of Europe, unfortunately, the committee was not complete at any of its meetings.

The Committee of Direction has made a contract with the house of D. Reimer & Co., of Berlin, which engages to undertake the publication of the map under excellent economic and scientific conditions. The map will be divided into 49 sheets, each sheet of 48 centimeters by 53 centimeters. These 49 sheets united will form a rectangle $3\frac{36}{100}$ meters high, $3\frac{72}{100}$ wide. Professor Kiepert, of Berlin, has agreed to prepare the topographic base, which will be entirely remodeled according to the most recent data that can be obtained. The house of D. Reimer & Co. undertakes the publication at its own cost, on the single condition that the international committee guarantee to it the placing of 900 copies at 100 francs a copy, and furnish it sums on account in advance. The price of subscription is 100 francs, but 125 francs will be fixed at as the trade price of the work. This guarantee subscription has been divided as follows. Each of the great States of Europe: Great Britain, France, Spain, Italy, Austro-Hungary, Germany, Scandinavia and Russia agree to take 100 copies each. The six small States, Belgium, Holland, Denmark, Switzerland, Portugal and Roumania, will divide among themselves the last hundred copies.

A promise has been received from each of the above named countries that it will lend its assistance to the committee, conformably to the distribution above, with the single exception of Spain, whose answer has not yet been received. The commission will consider what can be done to obtain this answer.

As to the geological symbolization, it will be furnished natu-

rally by the national committees, each one for its own country, and these contributions will be harmonized by the labor of the Committee of Direction, which, besides, will have the duty of completing the work, by all the data accessible to it, published or unpublished. The chromo-lithographic work will be done by the editors, Reimer & Co., conformably to the international scale fixed at Bologna and completed at this meeting.

The scale of the map was fixed by the unanimous consent of the Bologna Congress, September 29th and 30th, 1881, at $\frac{1}{1500000}$, at the same time that the map was decided upon, and its execution was confided to eight members composed of:

Messrs. Beyrich, Hauchecorne,	*Committee of Direction at Berlin.*	Germany.
Daubrée,		France.
Giordano,		Italy.
Dr. Möller,		Russia.
Mojsisovics,		Austro-Hungary.
Topley,		Great Britain.
Renevier (*General Secretary*),		Switzerland.

Specimens of the work done on the chart were exhibited to the Congress. The greatest progress had been made on those portions under the charge of Germany and Italy. The chart exhibited the wisdom of the decision of the Bologna Congress in expressing the successive subdivisions of the periods by graduated tints of the same color, the deepest tints representing the oldest stage.

At this point, Professor Capellini exhibited a roll that had been handed him as the first installment of the colored map of Italy, made on the scale agreed upon ($\frac{1}{1500000}$). It contained Central and Southern Italy.

M. Nikitin (Russia), reported that an installment of the map of Russia was *en route*, and that it would be exhibited the next day.

In conclusion, the report offered the following resolutions for the adoption of the Congress:

1st. Dr. Moeller, who has resigned, is hereby replaced in the commission by Mr. Karpinski.

2d. The "Carbonic" system (or Permo-carboniferous), will be represented by a gray color in three tints.

3d. The "Devonic" system will have three tints of brown.

4th. The color of the "Siluric" system is left to the choice of the committee on the chart.

5th. The eruptive rocks will be represented by seven tints of red, from bright to dark brownish.

6th. The determination of other questions in the report is left to the discretion of the committee on the map.

This was acted upon section by section. Section 1 was adopted without dissent. Section 2 was then read.

Professor Hughes (Cambridge), objected strongly. He said the discordance between the two formations in England was enormous and that the English geologists would never consent to this union.

Professor Dewalque (Belgium), defended the proposal of the committee.

M. Hauchecorne (Germany), urged that the gray chosen by the committee for the Permian was a greenish gray very different from that of the Carboniferous, and he believed that if Professor Hughes would look at the chart as made, he would find that all the distinction he desired was accomplished by this tint. His view was that a distinction of two entirely different tints of the same general color base would effect as complete a representation of the difference between the two series as could be effected by totally different colors.

M. Nikitin (Russia), thought the Carboniferous ought not and could not be joined to the Permian, and discussed the case of the so-called transition beds in Nebraska and elsewhere in illustration of the view.

Professor Renevier (Switzerland), thought the Culm, Carboniferous and Permian really constitute one system, but in order not to prejudge the case he had invented the term "Carbonic." Section 2 was then adopted. It was voted that the Carboniferous and Permian be colored in different tints of gray.

M. Dewalque (Belgium), objected to the use of the term Silurian in the following (4th) section, on the ground that the question of the limitation of the Silurian was to be brought up hereafter.

M. Renevier said he had used the term "Silurique" in order not to bring up the Silurian question, and moreover, he had said "Silurique, Cambrian included." He called the attention of M. Dewalque to the fact that it was impossible for him to discuss things without applying to them names, but that he did so in a manner that he thought would commit the committee and Congress in the least possible degree.

Professor Hughes energetically protested against the use of the word "Silurique." He had not found the Cambrian in the region of the Silures.

M. Jacquot (France) allied himself warmly with Professor Hughes in protesting against the use of the term Silurique, at least for the measures in France. One can recognize distinctly the difference between the Silurian and Cambrian in every part of the extended contact in his country, in the Pyrenees and in various other places they are never to be confounded.

Professor Renevier said, it is not a question of confounding

them, but it is simply a question of using one general color-base for a column of measures which have certain points of analogy and are usually found together. They could be easily distinguished from each other by differences of tint or other means.

M. Jacquot replied that he could not see any reason for uniting two things that are distinct.

M. Hauchecorne (Secretary), said: Gentlemen, we must get on, and I ask as a personal favor on behalf of the committee on the chart of Europe, that the members repose a certain amount of confidence in it. It is not intended to prejudge any question or force upon the delegates any views other than those they desire to support. He suggested that the fourth article might be so altered as to allow the committee to adopt provisionally according to their choice, a scheme of colors for convenience, and that this choice should not decide the scientific question connected therewith at all.

M. Jacquot accepted the suggestion of the Secretary, and thereupon section 4 was adopted.

5th. The eruptive rocks shall be represented by seven tints ranging from dark to light red. Carried.

6th. The solution of other questions that might arise were referred to the committee on the chart for decision. Carried.

M. Choffat (Portugal) said that in joining the Dogger and the Malm, a junction is made which is opposed by all paleontological and petrographical evidences.

M. Hauchecorne stated that in his opinion the objection was too much a matter of detail to be brought before the Congress at this time, and he appealed to M. Choffat to withdraw his objection.

M. Choffat replied that, in doing his work in Portugal, it was impossible for him to take this view of the two series.

M. Hauchecorne again appealed in the name of the committee to M. Choffat, to withdraw his objection, stating that the committees on the maps of Europe and of Portugal would have ample time to consider and adjust all these points of difference. No definite action was taken.

M. Hauchecorne then announced that the council would meet at 11 A. M. and the Congress at 2 P. M. on Wednesday. The first two hours of the session would be devoted to business and the last two to purely scientific discourses.

2D SESSION, 2:30 P. M., 30th SEPT.

At the request of Dr. Beyrich, the Honorary President, Dr. von Dechen, took the chair. The Secretary then made announcements in regard to excursions, and stated that the Congress until 4 P. M., would discuss the report of the committee

upon the chart of Europe. Afterwards they would listen to lectures upon special subjects: M. Gaudry upon certain reptiles, and Dr. Newberry (of New York), on a new fish from the Devonian.

The Secretary further announced the gifts which had been presented to the Congress.

M. Dewalque began the reading of the report of the committee upon uniformity of nomenclature at p. 13: A. Archæan System, Nos. 1, 2 and 3. "The first question to settle is, whether it should be included under the Paleozoic. The negative of this does not seem doubtful. Consequently and in conformity with the proposition of the French report, we propose to the Congress to decide that this system shall form a group to be known as the *Primitive* group. The termination of the word primitive will recall the characters which distinguish it from the groups '*primary, secondary,*'" &c.

M. Blanford proposed that we postpone the question of forming such a group till a later occasion.

Professor Hughes did not think that we had found the bottom of this group, and therefore we should wait for the determination of the term to be used, whether group or system. He called attention to an error in the report by which it would seem that the English committee prefers the term *Pre-Cambrian*. The English prefer the term *Archæan* to *Pre-Cambrian*, and they have used the former term.

M. Dewalque said if this group be not accepted, it must belong to the Paleozoic. [Loud objections.] Mr. Dewalque replied there was no way of avoiding the dilemma.

Professor Hughes thought we might represent it as a part of an unfinished system, but not as a system or a group.

M. de Lapparent (France) said if the Congress is willing to decide that there are no fossils in the Archæan, it should be set apart; if it contain fossils it must be joined to the Paleozoic.

M. Renevier proposed the term *Terrain* to avoid pre-judging the question of the rank in the classification of these rocks. He objected to the use of this term in any systematic sense, but believed it might be employed in a general sense.

Dr. von Dechen said, we want the terms "group" and "system" used for the chart, and do not want any vague terms. He believed it was necessary to maintain the usage of terms adopted by the Congress at Bologna.

Professor Hughes suggested that the use of the term *group*, for the Archæan be adopted, without settling its subdivision into systems, or attempting any correlation between subdivisions in different countries.

M. Renevier replied that we do not apply to eruptive rocks, the words "group" or "system," but simply "rocks." If erup-

tive rocks require no classification further than this, the words and coloration are sufficient for the Archæan.

Dr. Beyrich said that all that was necessary at present was the acceptance of the Archæan as anterior to Paleozoic time.

M. Stefanescu proposed the term "group" for all the rocks preceding the Paleozoic. His question was, "Shall we say Archæan 'system' or 'group?'"

M. Firket said there were two questions involved:

1st. Archæan or Primitive? 2d. Group or system?

M. Dewalque spoke to the same effect.

Dr. Hauchecorne asked for a vote on the terms "group" or system." "Group" was chosen.

The vote was then taken on "Archæan" or "Primitive." "Archæan" was adopted, after M. Renevier (chairman of the committee on the chart of Europe), had explained his views upon the question.

M. Dewalque proposed that some member should make a motion to divide the Archæan into three parts.

Dr. Hauchecorne asked M. Dewalque to make some proposition in order to bring the question before the Congress. No action was taken.

He stated that it was proposed to subdivide the Archæan into Azoic schists, Crystalline schists, and Protogine schists.

Professor Hughes suggested that it would be better to express the petrographic character and not divide the group chronologically. To this M. Renevier agreed.

M. Jacquot stated that no division of the Archæan in France was possible at present. The work of M. Lory in France and in the Alps results in establishing, as the best procedure, the coloration of mica-schists and gneisses in the same manner. He also supported M. Hughes's proposition.

M. Firket agreed to the petrographic divisions, but objected to the term "Azoic." It begs the question of the existence of life.

M. Stefanescu stated that the Archæan is well represented in Roumania, but there was extreme difficulty in making subdivisions.

M. Lapparent respectfully requested that the term "Protogine" be suppressed once for all, and gave his reasons. A vote was taken and the term was suppressed.

The proposition of Professor Hughes was then adopted, viz: to accept the Archæan as a group, leaving the petrographic divisions to each geologist and not assigning to them any chronological value.

The question then taken up was B 4, 5, and 6 (p. 14 of the committee's report), as follows:

The conference at Zurich has admitted provisionally the

union into one system (for which the name is yet to be determined), of the different beds, corresponding to the Cambrian and Silurian of the British Isles. The French, Portuguese and Roumanian committees propose the name of Silurian System. Before voting on the proposition, the Congress will have first to pronounce upon the names to be given to the three groups, and then on their union into one or two systems. The Hungarian Committee propose a Cambrian and a Silurian system; the latter comprising the groups 5 and 6 united. The Belgian Committee would have proposed an analogous grouping, but preferred to conform to the decision taken by a large majority at Zurich. The French Committee does not propose any name for the three groups. The Roumanian Committee gives them inadmissible names (these should be each in one word), "lower, middle and upper.' The Belgian Committee proposes the names Cambrian, Ordovician and Silurian. The Portuguese substitute 'Bohemian' for the last term. We have already said the English Committee has not been called to decide upon the questions of the report which have been submitted to it."

Since the receipt of the reports of the national committees, the questions to be decided have been complicated. M. Jules Marcou, in an important work published by the American Academy of Science and Arts, and entitled "The Taconic System and its position in stratigraphic geology," has vindicated the priority of the term *Taconic* of which the Cambrian alone (or Primordial fauna) would be the equivalent. We think the question is demonstrated. In such a case the term Cambrian would be employed to replace the Ordovician, the name Silurian would come back by right to group 6. If we be not in error this solution would avoid many difficulties. We propose to the Congress to determine first, the names that the groups 4, 5 and 6 should bear. It will have to decide afterward whether they constitute one or two systems, and finally the name or names to be employed.

Professor A. Geikie proposed that the Congress postpone the subject of subdividing the Cambrian and Silurian until the meeting in England; on the ground that the Silurian question was mainly an English question. (Loud murmurs). Professor Hughes agreed with Professor Geikie as to the propriety of postponing the discussion of these questions, and said that Professor Hall had also expressed his approval of this course.

The chairman, Dr. von Dechen, put the question to divide the Silurian, but leave the names till the meeting in England. M. Capellini regretted such action, if it would postpone the completion of the European map. M. Hauchecorne said it would not, as the map could be completed without waiting for the determination of the names.

The motion was then put and carried.

The Congress then took up the Devonian. M. Dewalque continued reading that part of the report in regard to the Devonian (pp. 15, 16), numbers 7, 8 and 9. (*a.*) Conformably with the only propositions that have been made, the Congress is requested to decide that the three series of this system shall bear the names respectively of the Rhenian, the Eifelian, and the Fammenian.

(*b.*) We propose that the Calceola beds should form a part of the Eifelian.

(*c.*) Finally we propose to the Congress to decide that the upper limit of the Devonian system is to be placed at the base of the Carboniferous limestone; that is to say, that the system comprises the psammites of Condroz, the lower Carboniferous, (Kiltorkan, Marwook, Pilton) the upper 'Old Red' or the Calciferous sandstone, etc.

M. Renevier asked why the Coblentzian was called Rhenian. M. Lapparent explained that Coblentzian was used in a more restricted sense.

M. Dupont demanded that the upper Devonian begin with the zone of *Cyrtia Murchisoniana*. Dr. Beyrich remarked that few in Germany would agree to this classification. M. Renevier desired to say that M. Gosselet, whom he had expected to see here, regarded the junction between the Calceola beds and the Stringocephalus beds as forming the division between the lower and middle Devonian. M. Dupont remarked that such was the classification of M. Gosselet some time ago. Much had been done since. Dr. von Dechen said the Calceola beds should be in the middle Devonian.

The third section of the Devonian (in regard to its upper limit) was then read.

M. Geikie said that an error had crept into this clause and he proposed to strike out all after the word 'Condroz' except the words "the Upper Old Red." M. Renevier objected to sharp lines. We should not go into such details and he asked for the striking out of the clause on principle. M. Capellini said if it was necessary for the coloration of the map he would retain it, but it did not seem to him to be necessary. M. Dewalque thought it was necessary to make sharp distinctions in the map as to the beginning and ending of series, otherwise how was it possible to compare corresponding series in different countries? The limits must be at the same horizon for all regions recorded on the map.

M. Capellini proposed to adjourn the decision of fixed limits, because it was not necessary to the coloring of the chart. M. Hauchecorne was of M. Dewalque's opinion. Dr. Beyrich thought that we could not separate the Devonian from the Carboniferous at an absolute horizon.

M. Renevier said this was necessary in important cases but was not important here. M. Lapparent remarked that if the geologists of England are content to sit still and make no objection to the proposed limitation, the Congress would save much trouble by permitting the proposed limitation to be accepted, because the English are most interested in it.

At the close of the discussion the clause was stricken out.

Several scientific papers were then read, among them one by M. Gaudry on some fossil reptiles, and another by Dr. Newberry on a new Devonian Fish.

3D SESSION, OCT. 1, 2:30 P. M.

Session opened with Dr. von Dechen in the chair. M. Fontannes, secretary, read the minutes of the last two meetings, after which M. Dewalque continued the reading of the report of the committee on unifying the nomenclature. Professor Capellini read a telegram from the Syndic of Bologna as follows: "Bologna, proud of having been the seat of the second session of the International Geological Congress, sends an affectionate greeting to the illustrious savants assembled at Berlin, and hopes that their works will aid the progress of civilization."

M. Hauchecorne then announced the scientific memoirs which would be presented at 4 P. M.

Szabo: On the new map of Schemnitz.

Mayer-Eymar: The perihelions of the Globe and the sedimentary rocks.

Reusch (Norway:) 1. Exhibition of a meteorite which fell in Norway in 1884, with some observations on meteorites in general.

Reusch (Norway:) 2. Exhibition of specimens and charts illustrating the phenomena of pressure and tension in metamorphic rocks.

Taramelli: On chemical deposits, and two or three other papers.

This was followed by a list of the donations given to the members of the Congress and to individuals.

The announcement of the trips to Thale, Leipzig and Stassfurt were so modified as to enable those members who desired to proceed directly on Wednesday to Dresden instead of remaining to make the geological excursions with Professor Credner. These would go directly to Dresden under the auspices of Geheimrath Professor Geinitz, and on Thursday visit the Natural History Museum of the Zwinger, and afterward the collections of the Royal Gallery. Thursday evening they were to reassemble on the Brühlische Terrasse, and the next day to spend the time in observing the collections of Dresden. They will reassemble on the Belvidere on Friday evening. (This programme was carried out with some modifications.)

The continuation of the Report of the committee on nomen-

clature was then proceeded with by M. Dewalque at p. 15. D. the Carboniferous System.

M. de Lapparent took the floor and supported the proposition of the committee to unite the Permian with the Carboniferous. His ground was that every classification should base its horizons upon established fauna. Most happily for the geologists, in the earlier formations there is the most valuable evolution of the Cephalopods; but it was not thus with the Permian, as he appealed to the paleontologists who had occupied themselves with these beds to declare. Among other arguments presented, he remarked that in Asia, there was no Pelagic fauna, by means of which one could distinguish the Carboniferous and the Permian, and the same was true in other countries of which there were representatives present. He concluded, "I believe that in establishing the Permian as a unit we construct something which has nothing in common with the characters adopted for other sub-divisions; which has no distinctive characters of its own; and which in fact does not exist. Whereas united to the Carboniferous we have two distinct horizons of faunas, each of which is susceptible of further subdivision by pronounced differences in character. Dr. Beyrich made some observations. Mr. Jacquot thought that Mr. Dewalque should read to the Congress the opinions that had been expressed by the different national committees. This would have, in his view, the most capital importance in deciding the question. Mr. Dewalque conformably to the request of the last speaker, called first upon the French committee.

M. Lapparent did not think that his opinion should be brought into conflict with that of the French committee, to which as a member his name was attached.

M. Renevier spoke on this question.

M. Choffat, in the course of his remarks, insisted that the question of the thickness of measures was an entirely insignificant one.

M. Capellini read the report of the French Committee and observed that M. Lapparent may very well present his own views in the Congress, even though they be different from those of the committee.

Prof. Hughes exhibited a chart of a section made by himself: there was a large gap between the Permian and the Carboniferous; still the amount of time to be ascribed to that gap is different in different places, and no doubt if the contact line could be every where examined, places would be found where the two systems would approach each other very nearly. As at the base of the Carboniferous also, there is an enormous break of at least 27,000 feet of measures that had been eroded before the present discordant contact was effected. That between the Permian and

Carboniferous represents also an enormous lapse of time. In reply to the argument from the percentage of fossils common to the Carboniferous and Permian, he observed that the number of fossils, which are found in a given neighborhood depends both upon the excellence of the geologists looking for them and the assiduity of their search. The percentage of fossils common to the Paleozoic and Mesozoic is increasing every day in proportion to the hammering done.

M. de Lapparent was of the opinion that the arguments for establishing these stages should be pelagic traces rather than geographic situations. He continued, that if we could restore the geographical divisions of the world as they were at the time when these various groups were laid down and the Carboniferous and Permian did not present analogies which could be made out, he (Lapparent) would acknowledge himself in error, but the same argument could not be drawn from the present geographic conditions of the earth. He would cite, however, another argument, namely that from petrographic studies. There was not to be found in the Permian a trace of certain rocks so peculiar throughout the Carboniferous. All the outflows characterizing the Carboniferous on the one hand and the Triassic on the other were wanting in the Permian measures, where another order of things from that preceding seemed to have supervened.

Dr. Beyrich made some remarks. Another member of the Congress, stated that the Rothliegendes must be separated from the Carbonic and also from the Triassic.

The Hon. President, von Dechen, said that the Rothliegendes was a very remarkable group. It has the thickness in some places of 1,600 meters, and even at this depth the bottom is not found. Rothliegendes and Zechstein occur over vast extents of country. In Russia there are outcrops of it larger than the whole of some countries existing in Europe.

M. Blanford said: "In taking up this question we take up one that concerns many parts of the world. Outside of Europe there is no Permian—I mean no European Permian. It is impossible to separate the upper from the middle and lower Carboniferous. I believe that the fauna of the Zechstein is a local fauna and therefore I give my adhesion to the views of M. de Lapparent as to uniting the Permian and the Carboniferous."

M. Capellini, rising with the report of the committee at Zurich in his hands, remarked that there must be some mistake in the printed report inasmuch as it was there stated that M. Blanford was of the opinion that there was an evident division between the Carboniferous and Permian.

M. Blanford stated that the report was entirely correct and that he would explain how the misunderstanding arose. He was under the impression, during the discussion at Zurich, that

the question was simply of European geology—and in the vote that was taken he had no part.

M. Stur made some observations on the course to be pursued in treating these two formations. He believed in uniting the Permian and Carboniferous in one system.

M. Nikitin: "We have two regions in Russia where we have studied these groups. They are divided into two stages. In central Russia, in the Volga valley, we can distinguish them, but at the foot of the mountains we cannot. We cannot at the present time, therefore, define accurately the limits between these different systems, but no doubt in the future we shall be able to do so.

M. Renevier was glad to hear from M. Stur's remarks the confirmation of views which he had always held and often expressed, namely: that the classification based on gaps is false and artificial. I agree with M. Nikitin, that our groups are all artificial. (Dissenting murmurs). Oswald Heer calls the Permian Upper Carboniferous by its flora. And as to the fauna he has shown a great number of species that are similar. M. Gaudry has done the same for the reptiles; M. Fritsch's views tend in the same direction. The divisions ought to be made on paleontological evidence.

Professor Newberry remarked that he knew it was a question here of the European map, and perhaps it would be an impertinence on the part of an American to express any opinion; "but I am asked," he continued "to express the opinion of my honored colleague Professor Hall, that there is no Permian in America. From my own studies also I know, that there is an insensible transition from the Carboniferous beds, to those which correspond in position to the Permian, and there is no strict line of demarcation between the Trias and the Permian. Therefore, for America (and only for America I speak), the Permian as a separate division does not exist."

M. Capellini: "The president asked me to see what can be done to advance the map, and although it appears to me that a majority of those present is in favor of joining the Permian and Carboniferous, still there is a respectable number of those who are opposed to it. And therefore the commission on the map would propose to adjourn the discussion and definite settlement of this question until a future time.

M. Topley said: "M. Blanford speaks only in general terms and not for England in the matter of these groups. It is highly important, as well for the classification as for the economic geology of England, to preserve the identity of each system. He agreed with Professor Hughes in drawing a strong line of demarcation between the Permian and the Carboniferous.

M. de Lapparent said: I ask the Congress to give the statements made by Professor Newberry and M. Blanford, in regard to the absence of the Permian in various parts of the world, the attention that they deserve. It seems to me that the object of this Congress was to establish a system applicable to all the world and not to Europe alone, or it should not have invited geologists from other countries than the European to participate.

M. Capellini remarked that these matters were to be settled as broad and grand questions in Science—without paying too much attention to individual matters of detail in which different countries might differ.

M. Neumayr thought that just because the questions were grand and broad they should be left to the free and unrestricted discussion of scientific men in the journals and societies of the world, and not be settled by a majority which changes with every country, and after the address of an eloquent orator.

M. Capellini made some further observations. Professor Hughes said that Professors Newberry and Blanford had stated that there was no Permian in India and America, but that they had simply found fossils having a Permian facies in the Carboniferous. He concluded by expressing the belief that it was better to leave the question open.

M. Hauchecorne: I agree entirely with the views of Professor Hughes as to the scientific aspect of the question, which we propose to leave to the future. But in the map we will arrange the order of the beds provisionally as it is in the proposed chart of colors without uniting the two systems in the legend of the chart by a bracket.

The Hon. President von Dechen agreed with the views expressed by Mr. Neumayr and desired the map to go on to its completion at the earliest moment.

M. Dewalque: "I propose the following as expressing the opinion of the Congress on this subject:"

"The Congress not wishing to pronounce any view on the scientific question of the proper division of the Permian and Carboniferous, preserves the classification as it now is." (Adopted with about fifteen dissenting votes).

4TH SESSION, OCT. 2D, 2:30 P. M.

The Congress assembled in the Reichstags chamber and M. Capellini occupied the chair as chairman *pro tem.*

The report of the Council was read and the nominations proposed by it for the committee on the chart of Europe were voted upon and unanimously elected. They were as follows in alphabetical order (in French) by countries:

Germany,	Römer.	India,	Blanford.
Austria,	Neumayr.	Italy,	Capellini.
Belgium,	Dewalque.	Japan,	Neumann.

Canada,	T. Sterry Hunt.	Norway,	Kjerulf.
Denmark,	Johnstrup.	Holland,	van Calker.
Spain,	Vilanova.	Portugal,	Choffat.
United States,	James Hall.	Roumania,	Stefanescu.
France,	de Lapparent.	Russia,	Inostranzeff.
Great Britain,	Hughes.	Sweden,	Torell.
Hungary,	Szabó.	Switzerland,	Renevier.

The members of this committee were requested to vote for a president for the next meeting. M. Fontannes read the journal of the preceding session, which was approved.

M. Capellini, in the chair, then took up the question on which the Congress was engaged at the close of the last session, and asked if any one wished to speak further upon giving three divisions to the Trias. After a pause, M. Renevier remarked that he did not wish to take up the time of the Congress, but he wanted to know how it is intended to color the Trias. Is it intended only provisionally to accept the divisions for the chart or not?

M. Stefanescu said the proposition to accept the divisions of the chart prejudges the whole question.

M. Dewalque, M. Blanford, and M Capellini further discussed the question, and finally the three-fold divisions of the Trias proposed at the Zurich meeting was agreed to.

The question as to the proper place of the Hettangian beds (whether with the Trias or with the Lias) was discussed but no decision was reached.

It was decided to divide the Jurassic into three parts.

The question of the union of the Rhætic, not including the Hettangian, with the Lias or Trias was again discussed.

M. Hauchecorne observed that the scale of colors and symbols were so arranged that the Rhætic could be classed with the Trias or Lias to suit the observer. The question as to the superior limitation of the Lias with the zone of *Ammonites opalinus* was discussed.

M. Choffat thought that so little of this series is known in Europe that the limit should be left for each geologist to place it at his own discretion. Agreed to.

Next the Tertiary was taken up. An animated debate ensued in which M. Meyer of Zurich, Dr. Beyrich, M. Stefanescu and M. Neumayer took part.

Finally the chairman, M. Capellini, proposed that, in view of the fact that no progress seemed possible owing to the divergence of views maintained, a vote of confidence in the committee on the chart be taken; assuring the members of the congress that the committee would exhaust every means to satisfy the views of the different members. (This vote of confidence was carried unanimously.)

The proposition in regard to the Eruptive Rocks was then taken up.

Professor von Dechen declared that there should be no distinction made between the rocks of extinct and of active volcanoes, or between ancient and modern eruptive rocks, but there should be a strong distinction drawn between ancient tuff and ancient eruptive, and between modern tuff and modern eruptive rocks proper.

Dr. Beyrich agreed with his Excellency, Dr. von Dechen, on this point.

M. Blanford said it should be taken into consideration that in parts of England, in the Hebrides, in parts of America and elsewhere, there were eruptive rocks, and lavas which resembled stratified rocks very closely. He objected to the petrographic division of the eruptive rocks, while the sedimentary rocks are divided chronologically; the more so, as many of the eruptive rocks, like those he has instanced, strongly resemble the stratified rocks.

The whole matter was finally left to the committee on the chart.

The president pro tem. then passed to the second order of business, and gave the floor to Dr. Neumayr, who read the report upon the proposed plan for the preparation of his Nomenclator Paleontologicus.

5TH SESSION, OCT. 3D, 10 A. M.

At the morning session several scientific papers were read. Among them was a report upon the system of coloration in use in the United States Geological survey. Mr. McGee, who had prepared this report, did not arrive till late in the progress of the Congress. The paper was presented to the council in English, but the rule requiring all the communications to the Congress to be presented in French, necessitated the preparation of an abstract in that language. This abstract, at the request of Mr. McGee, was presented to the Congress by Dr. Frazer, together with prefatory and explanatory observations by himself regarding the map; exhibiting the principal features of the system which was displayed.

At 2.30 P. M. the sixth and closing session of the Congress was called to order. The journal of the last sitting was read and approved. M. Hauchecorne made several amendments.

Three sheets of the map of Galicia were presented, with a letter from their author, Professor Szajnocha. A letter was received from M. Abich, stating that he had returned to St. Petersburg and had resumed his labors.

M. Capellini (pres. pro tem.) called attention to the Nomenclator Paleontologicus, of which M. Neumayr had given description yesterday, and recommended that it be published under the

auspices of the Congress and under the editorial direction of a committee, consisting of Messrs. Gaudry, Neumayr, Zittel and Etheridge, with power to add to their number. (Carried.)

M. Vilanova then mounted the tribune and asked assistance for his polyglot dictionary of geology, a Spanish-French specimen of which he exhibited.

The committee on the formation of an international geological society, and of an international geological journal reported, and a letter was read from M. Gregorio of the committee favoring the plan; whereupon the president pro tem., M. Capellini, stated that upon consideration of the report and the facts, the council had decided against the advisibility of both plans.

Baron Levi asked an explanation. M. Capellini stated that no reports could be made to the Congress, unless previously recommended by the council, and explained that it was not intended to slight the proposition of his countryman, whose acts and motives were warmly approved and appreciated, but simply to adjourn the question till the meeting of the next Congress. Upon this a vote was taken upon the action recommended by the council, viz: favoring the scheme of an international geological journal, provided it were supported by joint private enterprise, which was approved. The president pro tem. then announced that the second part of the programme would be proceeded with and gave the floor to M. Nikitin, who explained the work he had done on the portion of work in Russia committed to his care, viz: Central and South East Russia, including the basin of the Volga.

On the conclusion of M. Nikitin's remarks, M. Vasseur took the floor and exhibited thirteen sheets of the geological map of France, prepared according to the principles adopted at Bologna. M. Hauchecorne, the general secretary, stated that it was a pity that the legend of the Russian maps should be printed in characters which people of other nationalities could not understand, and he asked that a copy of each map should be furnished with the names in French characters. M. Nikitin replied that every sheet that he had exhibited contained the names of all the important places and all the rivers and streams in French characters, and demonstrated that this was the case.

M. Posepny read a treatise on the fluid condition of the interior of the earth. M. Ochsenius presented his views on the origin of salt deposits and gave diagrams and explanations, claiming analogies between certain chemical and physical conditions in the Caspian and the German oceans and the results of explorations to be seen in the mines at Stassfurt and elsewhere.

M. Capellini (president pro tem.) then announced that the hour had come to draw the session to a close.

It was time for the Congress to determine the place of meeting of the 4th Congress of 1888. The council had to propose that the next meeting be held in the year 1888, between the fifteenth day of August and the fifteenth day of September; that London be the place of meeting and that Messrs. Geikie, Blanford, Hughes, and Topley be the committee to prepare for the proper reception of this Congress. Professor Hughes thought it had been very appropriate to cede to Germany the place of meeting of the present Congress, and its success had justified his opinion. He repeated his statement made to the council, that he had a petition signed by one hundred and thirty-seven English geologists requesting the Congress to meet in London. This petition included the names of the Duke of Argyle, the Earl of Enniskillen, and some of the most eminent geologists of England; and he hoped that England would be chosen as the next place of meeting.

M. Geikie expressed the same views and said that English geologists follow the action of this Congress with the greatest interest, and would unite in giving it a warm reception.

The recommendation of the council was approved.

The acting president, M. Capellini, yielded the chair to the president, Dr. Beyrich. M. Capellini then took the floor and said: "Before parting, thanks were due to certain august personages and societies and individuals, naming His Majesty, the Emperor of Germany; the Prussian Government, and especially the Minister of Public Works, and the Cultus Minister who opened the Congress with an able address, the Academy of Mines, his Excellency Dr. von Dechen, Dr. Beyrich, and Dr. Hauchecorne." (Applause).

Dr. Beyrich observed that in the last words he had to address to the Congress, he begged to be permitted to speak in the language in which he thought. He thanked the Congress for its kind assistance and support, and introduced his Excellency, Dr. von Dechen.

The honorary president remembered well the first scientific Congress held in Berlin in 1858, under the auspices of the Baron Alexander von Humboldt. Berlin was then a small town but had grown enormously since. He concluded by hoping that all the members would return to their homes with an agreeable souvenir of their sojourn in Berlin.

Dr. Hauchecorne, the general secretary, spoke of the eminent service of M. Capellini, and concluded with the hope that the friendships made here would endure and be the more closely knit at the future session to be held in London.

M. de Lapparent mounted the tribune and expressed, on behalf of the members of the Congress, their sense of obligation to the German committee of arrangements. Geological questions, he

said, were of a kind to be settled on the spot, and geological brethren mutually dug in the earth and divided the debris in a christian spirit. While here in Berlin, our intellects, our artistic tastes, and our capacities for pleasure have all been considered. Honor to the noble science of geology, which can induce intelligent men such as our hosts, to provide for the dead fossils from the earth's crust mansions as superb as the residences of kings. (Applause.)

The Congress was thereupon declared adjourned.

Art. LVIII.—*Bright Lines in Stellar Spectra;* by O. T. Sherman.

Up to date, as far as my knowledge goes, bright lines have been admitted to form part of the spectra of but six stars, β Lyræ, γ Cassiopeiæ, and four small stars in Cygnus. The claims of four others in Orion have been advanced and denied.

In the recent volumes of the Nachrichten, Konkoly and Gothard have called attention to the first and second of the stars enumerated above. The result of the former work may be summed up in the identification of the bright lines D_3, $H\alpha$, $H\beta$, $H\gamma$, $H\delta$, of the dark lines b, D, and a broad band in the violet, and the recognition of a seven day period for the variation of the spectrum of β Lyræ.* Konkoly also says:† "Ich glaube auch noch manchmal im Grün und Blau einige sehr zarte Linien gesehen zu haben, was aber auch eine durch das Flattern des Spectrums verursachte Täuschung sein kann." With a view to following these stars and learning whatever a conscientious study of their spectra might show, the equatorial (8 in.) of the Yale College Observatory was devoted thereto.

The spectroscope employed is a direct vision by Duboscq; the distance from the slit to the collimating lens being about 14·8^{cm}. The train is broken into two series of three prisms each. Using the single series the lines b_3 and b_4 are barely separated. Using the double series the nickel line between D_1 and D_2 is seen, and b_3 is separated from b_4 by about the width of the latter. The power of the eyepiece of the observing telescope is about 130. A cylindrical lens behind the eyepiece is usually employed. Previous to each night's work the instrument was adjusted upon the sun; a solar spot, when possible, being brought sharply in focus upon the jaws of the slit. For stellar observation the slit was opened wide, 5^{mm} or more.

* Astronomische Nachrichten, 2539, 2548, 2651, 2581.
† Astron. Nachr., 2548, p. 62.

When the series of observations was first commenced there were recognized in the spectra but a few bright lines, so situated as to be probably the hydrogen lines, D_3, and in addition 1474(K) and 1250(K). As the observer became accustomed to the spectrum of β Lyræ it became apparent that there were also other bright lines. With the single series of prisms ten such were counted.

Recalling now the course of reasoning which led to the daylight observation of the solar prominence, and also that many more bright lines than those already recognized were seen in the spectrum of the solar atmosphere, I employed the highest dispersion obtainable. The number of bright lines was increased to seventeen. It seems extremely probable that an increased dispersion will bring out many more. Arrangements for so improving the apparatus are in progress. The story for γ Cassiopeiæ is similar.

The instrument has been turned upon numerous other stars and in each case many or few bright lines have been seen, lines, so far as I know, formerly unsuspected. The careful description awaits the completed apparatus. At present it would seem that the lines are most easily seen in the red stars. This may be a mistake. The word lines is here used only by analogy to signify bright stellar images. At the red end under a sharp focus they stand out the full breadth of the spectrum, bearing somewhat the same relation to the background as the prominence to the solar spectrum. In the brighter portion of the spectrum they are cut down to fine star points. At the blue end they become more distinct but not so sharp as at the red. At times they shine with almost a metallic brilliancy; at other times they are faint, faded, and easily passed over. Certain sets appear to be prominent at times, others at other times.

The difficulties of the observation, and the roughness of the recording apparatus have hindered a completely satisfactory identification of the lines. Assuming the position of the hydrogen lines and of D_3, and on their basis drawing the curve connecting scale reading and wave length, the mean of nine observations upon γ Cassiopeiæ affords the following approximate wave lengths. The positions of the dark lines are underlined.

$H\alpha$, 635·6, 628, 616, D_3, 584·0 ? 576, 555·75, 542·2, 530·98, 516·75, 502, 499·0, 492, $H\beta$, 467·35, 462·3, $H\gamma$ 418 ? $H\delta$, 399·3.

It is of interest to compare these with the following wave lengths taken from Prof. Young's catalogue of lines observed at Sherman:*

$H\alpha$, 634·6, 614·06 D_3, 585·27, 553·4, 544·59, 531·59, 516·83 and 516·67, 501·76, $H\beta$, 457, $H\gamma$, 421·5, $H\delta$.

* This Journal, Nov., 1872.

While the identification is not complete the number of the approximate coincidences renders it extremely probable that the lines observed are those of the solar atmosphere.

To quote the pioneer of American spectroscopic observations, it would seem that there are many stars in the same condition as the sun, but with the corona more pronounced.

Yale College Observatory, November, 1885.

Art. LIX. — *Note on the Optical Properties of Rock-salt;* by S. P. Langley.

Since the first experiments of Melloni the optical properties of rock-salt have received comparatively little attention, although this substance is every day coming more into use, as the importance of the study of radiant heat is recognized. It was asserted by Melloni that rock-salt is almost perfectly diathermanous to all kinds of heat radiations, and that it transmits a little over 92 per cent. of the incident heat of whatever kind. This statement was disputed by Provostaye and Desains,* who maintained that there was considerable difference in its absorptive action toward heat radiated from sources differing widely in character; still, even admitting the claims of these physicists, the great difference between the action of this substance and others, such as glass, is very striking, and we are dependent chiefly on it for our analysis of the action of obscure heat.

In certain researches which have been made of late at the Allegheny Observatory on the distribution of heat in the spectrum of the moon, and of terrestrial sources of radiation at very low temperatures, the use of an exceptionally perfect rock-salt train has been sought in order that heat deviations measured with a precision comparable with that of optical work might be secured, at the same time that the extremely feeble radiations at command should suffer the least possible amount of absorption in the apparatus. After long searching, blocks of rock-salt were finally obtained through the great kindness of Professor C. S. Hastings, of Yale College, from which two 60° prisms were cut of about 64 millimeters on a side, and lenses of nearly 75^{mm} aperture. The most perfect prisms the writer could obtain in Europe, did not distinctly show a single Fraunhofer line, and he was assured by opticians there that no rock-salt prism ever did or could do more. He is happy to say that the skill of our American opticians has produced what was pronounced impossible,—a rock-salt prism which shows the Fraunhofer lines with all the sharpness of flint glass. Such prisms

* Comptes Rendus de l'Académie des Sciences, xxxvi, p. 84.

have been made for us by Messrs. Alvan Clark & Sons, of Cambridge, and Mr. J. A. Brashear, of Pittsburgh, who has worked that here described and which shows the nickel line between the D's.

For the reduction and arrangement of these observations a more complete knowledge of the properties of rock-salt than had hitherto been obtained was essential, particularly as regards its indices of refraction for rays of determinate wave-length, and its diathermancy for dark heat rays of different degrees of refrangibility.

Advantage has been taken of the use of the train above mentioned to determine, not only the indices, which will presently be given, but also the the apparent transmission of rock-salt plates in different parts of the spectrum, but the latter part of the work is not represented here. We shall only observe that we have had occasion to form "heat"-spectra from radiating sources below the temperature of melting ice, and that while most ofthe rays, even from these sources, passed freely through the prism; with the smallest deviations corresponding to wave lengths, probably exceeding 100,000 of Ångström's scale, a slight absorption began to be noticed. We hope to shortly give more full determinations of this, in connection with a statement of the deviations and wave-lengths of heat from sources at all degrees between the temperature of melting platinum and that of melting ice, with which it will appear in a more proper connection.

Although in working with such heat radiations even as those forming a part of the solar infra-red spectrum, more error is to be expected than in the optical observations, these errors are, with our present apparatus, of an order not wholly incomparable with the optical ones.

It is however to be understood that the best heat spectrum work can only be accomplished with *brightly polished* rock-salt. The surfaces of the rock-salt prism and lenses undergo a deterioration when exposed to the air, which is more or less rapid according to the greater or smaller relative humidity of the atmosphere at the time. In ordinary dry weather they may be used several times before they become spoiled, while in damp or rainy weather, three or four seconds is a sufficiently long time to cover them with condensed moisture, and work under these circumstances is of course impossible. After the surfaces have in this way become unfit for use they are repolished, and the refracting angle of the prism is thereby unavoidably altered. The change is usually small, generally not exceeding 1′, so that for most of our heat measures it may be neglected altogether. The changes have however tended on the whole to reduce the refracting angle, so that it is now about 4′ smaller than when the prism was first used.

To make all observations strictly comparable, they are reduced to one value of the refracting angle, for which the deviations of the Fraunhofer lines and the wave-lengths corresponding to given deviations in the infra-red, have been determined with the greatest possible accuracy. This standard value of the refracting angle is 59° 57′ 54″. A series of observations for fixing the positions of the Fraunhofer lines was made by Mr. J. E. Keeler of this observatory, on September 14th, 1885. One arm of the spectrometer, which was firmly clamped, carried a glass collimating lens of 25 feet focus, and the other an achromatic observing telescope of nearly four feet focus, with a micrometer eyepiece. The double deviations of the C, D_1, b_1, and F lines were observed, and also the differences of deviation between these and the other lines whose positions were determined. For observing the M and N lines a Soret fluorescent eyepiece was used, and in the infra-red a bolometer, having a single strip $\frac{1}{10}$ millimeter in width. In the two last cases the prism was automatically kept in the position for minimum deviation. The spectrometer circle reads by two opposite verniers to 10″, but on account of the construction of the instrument, (for whose principal purpose arms whose length is inconsistent with absolute rigidity had to be used) care is necessary to measure an angle with this degree of precision, as the arms are liable to spring slightly on the application of lateral pressure. The deviations given in our table were obtained by Mr. Keeler by setting on the line, with the micrometer eyepiece, after the telescope had been directed upon it and freed from strain by a light tap, and applying the micrometer correction to the circle reading. It was found by careful comparison of the solar spectrum given by the rock-salt prism with that by a fine prism of flint glass, that in spite of the greater dispersion of the latter, no lines could be seen in its spectrum which the rock-salt prism would not also show. The probable error of one setting of the micrometer was less than 1″. From the agreement of the different measurements made in this way, it is believed that the deviations throughout the visible spectrum are correct to within 5″. Those in the ultra-violet and infra-red, cannot of course pretend to this degree of accuracy. The positions of $\rho\sigma\tau$ (invisible) given by two independent series with the bolometer differed by 30″; those of φ (invisible) by 1′, those of ψ (invisible) by 30″ and those of Ω (invisible) agreed exactly.

We have thus obtained incidentally the data for constructing a table of refractive indices of rock-salt throughout the entire range of the solar spectrum, with an accuracy which we believe to be greater than has heretofore been attained, and which we deem of sufficient interest to give in full below, for the con-

venience of others having occasion to work with this material, and for testing theories of dispersion.

Refracting Angle of Prism=59° 57′ 54″.

Line.	Wave-length.	Deviation.	Δ	Refractive Index.
M	0·3727	43° 50′ 57″	1·21	1·57486
L	0·3820	43 35 27	1.20	1·57207
H_2	0·3933	43 19 32	1·19	1·56920
H_1	0·3968	43 14 44	1·19	1·56833
G	0·4303	42 36 7	1·16	1·56133
F	0·4861	41 51 47	1·13	1·55323
b_4	0·5167	41 33 43	1·12	1·54991
b_1	0·5183	41 32 52	1·12	1·54975
D_1	0·5789	41 2 41	1·10	1·54418
D_2	0·5895	41 2 29	1·10	1·54414
C	0·6562	40 42 56	1·09	1·54051
B	0·6867	40 35 49	1·09	1·53919
A	0·7601	40 22 25	1·08	1·53670
$\rho\sigma\tau$	0·94	40 1 26	1·07	1·5328
φ	1·13	39 49 11	1·06	1·5305
ψ	1.39	39 39 56	1·05	1·5287
Ω	1·32	39 29 21	1·05	1·5268

Temperature=24° C. Barometer 731·1^{mm}.

The wave-lengths of the M and L lines are from Cornu, those of the lines between H and A inclusive, from Ångström, and those of the infra-red bands from the Allegheny observations. The column headed Δ was prepared at the suggestion of Mr. Keeler, and is for the purpose of facilitating the reduction of observations made with a different prism angle from that for which the table is computed, and for which our wave-length curves are drawn. If we differentiate the ordinary formula for a prism

$$n=\frac{\sin\frac{1}{2}(A+d)}{\sin\frac{1}{2}A}$$

with respect to A, which we now regard as a variable, we have

$$\frac{dd}{dA}=\frac{n\cos\frac{1}{2}A}{\cos\frac{1}{2}(A+d)}-1=\Delta$$

or $dd=\Delta\, d\,A$.

The values of Δ for the different lines of the spectrum are readily computed from the table of deviations and refractive indices. To find, then, the deviation of a line after any re-polishing of the prism, we have merely to multiply the change of the angle by the approximate value of Δ taken from the table, and we obtain the change in the deviation of the line, and hence also the deviation required. Thus, if the new angle is found on measurement to be 59° 57′ 44″, $d\,A=-10''$, and

the deviation of the F line, (say), will have been changed by $-10'' \times 1{\cdot}13 = -11{\cdot}3''$. That is, the deviation of the F line is now 41° 51′ 36″. The reduction from the new to the standard angle is of course the reverse of this, and that the use of the table saves much labor in redetermining the constants of the prism will be understood when it is added that ours has been entirely refigured and repolished by the maker as many as ten times during the present year.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the Reaction of Barium sulphate on Sodium carbonate, under pressure.*—Spring has succeeded in effecting by means of pressure alone, a reaction between barium sulphate and sodium carbonate. An intimate mixture was made of one part of pure precipitated barium sulphate, thoroughly dried, with three parts of pure and dry sodium carbonate. About a gram of the mixture was submitted to the compression, the cylinder produced pulverized and extracted with water; and the insoluble residue analyzed to determine the amount of barium carbonate produced. After compressing the mixture under a pressure of 6,000 atmospheres for a few instants only, nearly one per cent of the barium sulphate had been transformed into carbonate. The uncompressed mixture gave only traces of barium carbonate. The cylinder from the first compression was pulverized and compressed anew. After four successive compressions the amount of carbonate produced rose to 4·78 per cent, and after six to 8·99 per cent, thus showing very clearly the value of renewing the surfaces of contact. If these cylinders after pressure be left to themselves for some time, the chemical action continues up to a period of fourteen days; the quantity of barium carbonate, produced in the cylinder submitted to six compressions, rising during this time to 10·89 per cent, thus throwing some light on the interesting question of diffusion in solids. Again, if these cylinders, after compression, be divided in halves and one half heated for three hours to 120°, it is found on analyzing these two halves that the barium carbonate formed has actually diminished during the heating; the percentage falling between one and two per cent. The author proposes now to study the reaction of sodium sulphate and barium carbonate under the same conditions.—*Bull. Soc. Ch.*, II, xliv, 166, Sept., 1885. G. F. B.

2. *On Sulphocyanuric acid.*—The ready conversion of cyanic into cyanuric ether, led Hofmann to attempt a similar transformation with sulphocyanic ether. And successfully, for on heating methyl sulphocyanate to 180°–185° for several hours it was converted into methyl sulphocyanurate, proved to be the normal

not the iso-acid. On attempting to repeat this process subsequently with carefully purified materials, no trace of the polymeric body could be found. The result was, however, readily effected, after a few drops of sulphuric or hydrochloric acid were added to the liquid before digestion. The ether is obtained in beautiful crystals which may be heated to 180° with water without change; though in presence of concentrated hydrogen chloride it splits at 100° into methyl mercaptan and cyanuric acid. Similar ethyl and amyl compounds were obtained. To obtain the sulphocyanuric acid, the methyl ether was mixed with sodium sulphide and heated in closed tubes to a temperature of 250° for three or four hours. The solution, after filtration, was treated with hydrogen chloride in excess, whereby the sulphocyanuric acid was precipitated as a yellow granular powder. It may be obtained pure by conversion into the sodium salt and reprecipitation. It is scarcely soluble in water, even boiling, and is also insoluble in alcohol, ether, benzene and nitrobenzene. It has the formula $(N{\equiv}C{-}SH)_3$. . The sodium, barium, silver, lead, copper, potassium, lithium, calcium, magnesium and other salts are described insoluble.—*Ber. Berl. Chem. Ges.*, xviii, 2196, Sept., 1885. G. F. B.

3. *On the Synthesis of Cocaine.*—Some months ago MERCK announced the production of a derivative of cocaine, benzoyl-ecgonine. He has now succeeded in re-introducing the methyl group into this derivative and in reproducing cocaine. For this purpose the benzoyl-ecgonine was heated with the theoretical quantity of methyl iodide and potassium hydrate in methyl alcohol, in sealed tubes to 100°. The product obtained was identical with the natural cocaine in all its physical and chemical properties.—*Ber. Berl. Chem. Ges.*, xviii, 2264, Sept., 1885. G. F. B.

4. *On Hydrogen Persulphide.*—SABATIER has examined with care the substance known as hydrogen persulphide with a view to fixing definitely its composition. As ordinarily obtained it is a reddish-yellow oily liquid, varying in composition from H_2S_6 to H_2S_{10}. This uncertainty of composition is due to the free sulphur dissolved in the persulphide, the liquid saturated at 18° having the composition H_2S_{10}. It contains also H_2S dissolved, from which it may easily be freed by placing it in vacuo. The persulphide was prepared by Thenard's method by allowing a fine stream of calcium polysulphide to flow into concentrated hydrogen chloride, both cooled to 10°. The yellowish liquid, well dried, was then distilled in vacuo, and afforded a clear brilliant, very limpid, mobile yellow liquid, having an extremely irritating odor. While retained in the bulbs which acted as the receivers, it was permanent; but it decomposed on decantation. On analysis, three samples gave as the sulphur in excess of H_2S 57·9, 59·2 and 58·9 per cent; or 58·7 as a mean. This corresponds nearly to the formula H_4S_5, which requires 58·5 per cent. From these results the author concludes that the true formula of hydrogen persulphide is H_2S_2 analogous to H_2O_2; the excess of sulphur being due to a partial decomposition in the process of distillation, the sul-

phur vapor being carried over with that of the persulphide.—*Bul. Soc. Ch.*, II, xliv, 169, Sept., 1885. G. F. B.

5. *On the Valence of Phosphorus.*—The valence of the element phosphorus has long been an open question. On the one side the compound PCl_5 has been regarded as proof of its pentad character; while on the other, this pentachloride has been considered a molecular compound consisting of a molecule of the terchloride united to a molecule of chlorine, $PCl_3 . Cl_2$. Even the oxychloride $POCl_3$, may be written either $O{=}P{\equiv}Cl$ or $Cl_2{=}P{-}O{-}Cl$ to accommodate the former or the latter view. Michaelis and La Coste have now thrown some light upon this question; and this for the first time from the purely chemical side. In 1882 the former of these chemists, in connection with Gleichmann, discovered a body of the composition $PO(C_6H_5)_3$, which he called triphenyl-phosphine oxide. It was prepared by warming the hydrate, which itself was obtained either by the action of sodium hydrate upon triphenyl-phosphine dibromide, or by acting with potassium chlorate and hydrogen chloride upon triphenyl-phosphine; processes analogous to those by which phosphorus oxychloride is formed either from the pentachloride or the trichloride. The substance is a solid body, of specific gravity 1·2124 at 22·6°, having a vapor density of 9·9. The authors have now succeeded in preparing another body having the empirical formula $(C_6H_5)_2PO$, by the action of phenol upon diphenyl phosphorous chloride. This therefore must have the constitution $(C_6H_5)_2 PO (C_6H_5)$. Hence if phosphorus is trivalent and its oxychloride is $Cl_2{=}P{-}O{-}Cl$, this compound must be identical with triphenyl-phosphine oxide above described. But these bodies, whose molecular magnitudes are both expressed by the formula $C_{18}H_{15} PO$, are radically different in physical as in chemical properties. Triphenyl-phosphine oxide as already stated, is a solid body fusing at 153·5, and completely indifferent to bromine, oxygen, sulphur, selenium, benzyl chloride and methyl iodide; while the isomeric phenoxyl-diphenyl-phosphine is a thick oily liquid, which readily combines not only with the elements above mentioned, but with the alkyl-halogens to form crystallizable addition products; a property characteristic of the compounds of trivalent phosphorus. The constitution of this phenoxyl-diphenyl-phosphine therefore can be expressed in a formula only by considering it as a derivative of an isomeric phosphorus oxychloride at present unknown, which contains trivalent phosphorus, thus:

Phenoxyldiphenyl-phosphine	*Triphenyl-phosphine oxide*
$(C_6H_5)_2\overset{III}{P}{-}O{-}C_6H_5$	$(C_6H_5)_3\overset{V}{P}{=}O$
Isophosphorus oxychloride	*Phosphorus oxychloride*
$Cl_2\overset{III}{P} . OCl$ (unknown.)	$Cl_3\overset{V}{P}{=}O$

Hence phosphorus and the other elements of that group must be considered quinquivalent, and the compounds into which these elements enter with a less valence, as unsaturated.—*Ber. Berl. Chem Ges.*, xviii, 2118, Sept., 1885. G. F. B.

6. *On the Measurement of the Resistance of Liquids.*—Two methods for measuring the resistance of liquids have been proposed, in both of which polarization is a minimum; one, the electrometer method suggested by Lippmann,* the other the alternating current method brought into use by Kohlrausch. Bouty and Foussereau have made comparative tests of these two methods for the purpose of determining their relative value. In the former, as modified by Foussereau,† the column of liquid is included in a closed circuit, containing also a battery and a known and adjustable metallic resistance. By means of a commutator, the terminals of a condenser may be alternately connected with two points in the liquid column or two in the metallic resistance, the difference of potential of these terminals—corresponding to the difference of potential of the points with which they are in contact—being determined by means of a Lippmann electrometer in the condenser circuit. By adjustment two points in the metallic circuit are found such that their difference of potential is exactly the same as that between the two points in the liquid. The resistance between the latter is then equal to that between the former, which is of course known. Bouty in employing this method uses a compensating battery in the condenser circuit, and has obtained very satisfactory results with it.‡ The experiments on the alternating current method, were made by means of a small Deprez generator revolving about 100 times per second. This current passed through a Wheatstone bridge, a sensitive telephone being used in place of the galvanometer. Large electrodes, 0·01 square meter each, were employed and the generator and resistances were carefully insulated. But it was found next to impossible to adjust the telephone to silence when metallic resistances formed the sides of the bridge; although the resistance coils used were by approved makers. With low values, a minimum sound could be distinguished; but as the resistance increased, this minimum sound became louder and less distinguishable. But, what is of more importance, when the bridge was balanced for sound, it was entirely unbalanced for the resistances; the error rising to 20 per cent even. This result is manifestly due to the fact that these resistance coils were not free from induction and the coefficient of self-induction with alternating currents became a serious matter. The sides of the bridge were then formed of liquid resistances. Three of these consisted of pairs of glass jars containing zinc sulphate solution and amalgamated zinc plates; a syphon between them regulating the resistance of each pair. The fourth consisted of a specially constructed liquid rheostat, made of two glass cylinders, one above the other, containing each a copper electrode of large surface immersed in copper sulphate solution. By means of a glass tube passing through the bottom of the upper jar, nearly to the bottom of the lower, communication is established between them. A glass rod passing through the tube serves to vary the liquid in the tube and so the resistance of the apparatus.

* C. R., lxxxiii, 192, 1876. † J. Phys., II, iv, 189, May, 1885.
‡ Ibid., II, i, 346, Aug., 1882, II, iii, 433, Aug., 1884.

At its upper end this rod carries an index moving over a graduated scale, calibrated by an electrometer. The resistance of this rheostat varies from 1200 to 5000 ohms. With these resistances in the bridge, the extinction of sound in the telephone was absolute. The liquids to be measured were placed on one side of the bridge, using platinum electrodes. With strong or only moderately weak solutions, an excellent balance could be obtained; but when these became more dilute, the error was considerable. With one thousandth solutions of magnesium chloride and potassium chloride, the difference between two consecutive measurements was 2½ per cent by this method; while with the electrometer method, the error was only one-third of one per cent. The authors believe, therefore, that for very dilute solutions, the electrometer method is preferable.—*J. Phys.*, II, iv, 419, Sept., 1885.

G. F. B.

7. *A method of precisely measuring the vibratory periods of tuning-forks.*—The third volume of the Memoirs of the National Academy of Sciences contains a paper by Professor A. M. Mayer embodying the results of a research recently carried on by him with funds from the Bache endowment. This research had as its object the elaboration of a method for measuring accurately the times of vibration of tuning-forks, and the determination of the laws of their vibrations with reference to the use of the tuning-fork as a chronoscope. The method employed was briefly to make a clock flash, at each second, a spark of induced electricity on a trace made by a style attached to the prong of the vibrating fork. To accomplish this the pendulum of the clock was armed with a triangular piece of platinum foil which each second cut through a globule of mercury contained in a small iron cup. To insure the best results, fresh mercury was taken with each experiment and the height of the mercury was adjusted by a screw collar in such a way as to make the globule as nearly as possible rigid and free from vibrations with each touch of the platinum point. The clock through this mercury connection was placed in the circuit of the primary coil of an inductorium, the current of which was given by a single voltaic cell. The tuning-fork, with one of its prongs armed with a light style of thin elastic copper foil, was screwed to a board with a hinge which with a screw-stop suitably placed allowed of its being inclined so that the style was just in contact with a smoked surface of paper wound on a rotating cylinder. The secondary circuit of the induction coil included the fork and cylinder. In the experiment the fork was raised on the hinge, set vibrating by a bow, and then depressed again, so that the style should write out its vibration on the smoked surface; at each second, as the platinum-pointed pendulum left the mercury, the primary circuit was completed and an induced current caused a spark from the point of the style, which made a single minute circular white spot on the blackened surface. The determination of the vibration-period of the fork is obviously given by counting the number of waves in the trace

and measuring the fraction of a wave with a microscope-micrometer. It was found to be essential to accuracy that the induction discharge should give a single spark only and that the spot made by it should bisect the trace of the fork. A series of experiments with discharges, obtained on a rapidly rotating surface of blackened paper with currents of various strengths, showed that the discharge is ordinarily complex, and consists of a shower of sparks producing a large number of spark-holes on the paper. The proper conditions to be fulfilled to give the single spark-hole with a given induction-coil can only be obtained by a series of experiments varying the strength of the primary current and the area of the condenser in the secondary. In the experiments described the primary coil was 150 feet in length, the secondary 8 miles, and a condenser of plates of glass with tin foil with 50 square inches of area were employed.

With the instrument which has been described a number of separate investigations were made. The first had to do with the question of the influence of varying amplitude on the time of vibration. With amplitudes varying in one case from 1·19^{mm} to 0·59, in another from 2·39 to 0·61, and a third from 2·07 to 0·78, no variation in vibration-period greater than ·05 of a vibration was noted. In a second series of experiments the effect of temperature was considered, and the result established with six Kœnig forks with Ut_2 and Ut_5 as extremes, that for all forks of the same steel and shape the effect of change of temperature was the same. A change of 1° F. produced a change of vibration-period of $\frac{1}{21061}$ part. In another series of experiments the law of the running down in the amplitude of a fork's vibration; and in another the numbers of vibrations per second of some European forks of various standards of pitch were determined. In the latter determinations the probable errors in one of the mean cases was estimated to be $\pm$·0053 of a vibration; in another $\pm$·004 of a vibration.

Professor Mayer discusses further the use of the apparatus described as a chronoscope, and gives the results of some experiments with it on the velocity of fowling-piece shot of various sizes with various charges of powder. The degree of uniformity of rate of rotation of the cylinder is shown to be immaterial, and further it is shown that no correction is needed for the weight of the tracing style nor for its scrape on the paper. With an A fork with 440 vibrations per second, it is stated that the number of vibrations can be determined by this method to at least $\frac{1}{100}$ of a vibration, and the time record consequently to $\frac{1}{44,000}$ of a second.

II. Geology and Natural History.

1. *Fourth Annual Report of the U. S. Geological Survey.* 1882–'83; by J. W. Powell, Director. 474 pp., royal octavo, Washington, 1884.—This fourth report of the U. S. Geological Survey, recently distributed, contains, like its predecessors, large

and important contributions to the science. After a review of the work in progress through the year, there are Reports on the Hawaiian Volcanoes, by Captain C. E. DUTTON (pp. 81–212); abstract of a Report on the Mining Geology of the Eureka District, Nevada, by JOSEPH S. CURTIS (pp. 225–251); Popular fallacies regarding the precious metal ore deposits, by ALBERT WILLIAMS, Jr. (pp. 253–271); A Review of the fossil Ostreidæ of North America, by Dr. C. A. WHITE (pp. 281–316); a sketch of the Life History of the Oyster, by J. A. RYDER (pp. 315–334); A Geological reconnaissance in Southern Oregon, by ISRAEL C. RUSSELL (pp. 438–464). Captain Dutton's report is an elaborate memoir on the Geology of the Hawaiian Islands, and a very valuable contribution to the subject of volcanoes, prepared after a careful study of the region. To give any adequate abstract of the memoir would require the space of an article. All the reports are well illustrated by maps, sections, cuts, or plates.

2. *Geological Sketches of the Precious Metal deposits of the Western United States*, by S. F. EMMONS and G. F. BECKER. *With notes on Lead Smelting at Leadville.* 296 pp. 4to. From the 10th U. S. Census Report, vol. xiii. Washington, 1885.—This report is a popular, but not less a scientific, account of the great Western mining regions; and it is well adapted to its place in the Census report. It reviews the geological structure, mining deposits and mines for each of the States and territories in the west, giving sections of deposits, and tables mentioning the rocks and minerals of the various mines. The geologist and those interested in mines and the associations of ores, and in the sources of mining wealth in the country will find the report full of interest. The authors know well their subject, and describe the regions largely from personal study.

3. *Malesia: Plantæ Ospitatrici.*—In a former number (p. 245) we gave an abstract of Beccari's investigations and illustrations of those singular plants which, by an organic change of structure supply food and lodging to certain species of ants. The third part of the second volume of *Malesia* has now come to hand. It is wholly occupied with this subject, and illustrates it by 29 more plates of *Myrmecodia* and *Hydnophytum*, several of them showing the formicaries. Thirty species of *Hydnophytum* are here characterized, along with two more species of *Myrmecodia.* A general review is made of this group of *Rubiaceæ*, of the structure of the tubers, the formation of the galleries, their internal conformation, and of the ants that live in them, thus completing a monograph of the subject so far as these hospitable *Rubiaceæ* are concerned. A. G.

4. *Illustrationes Floræ Atlanticæ.*—The Flora of the French possessions in Northern Africa is making good progress, at least in the illustrations. The energetic Dr. Cosson brought out the first fasciculus of these fine imperial quarto plates, with their letter-press, in 1882. We have now received the second fasciculus, with the date of 1884. The plates (tab. 26 to 50) are all of

Cruciferæ, and the majority of the Brassicaceous tribe, of which the Mediterranean region is the special home. We have admirable illustrations of *Henophyton*, Coss. and Durieu (which would be *Oudneya*, so long obscure, except for a slip on the part of Robert Brown in citing a very different plant as a representative of it), *Reboudia* of the same authors, *Hemicrambe* of Webb, and *Cossonia* of Durieu, in three species, two of them here figured, all Brassicaceous types, along with curious species of *Sinapis*, *Erucaria*, *Enarthrocarpus*, etc. The remainder are siliculose genera, among them *Savignya* of the lamented Boisser. The figures were drawn by Cuisin, and are excellent. A. G.

5. *Physiological Botany.* I. *The Outline of the Histology of Phænogamous Plants.* II. *Vegetable Physiology;* by GEORGE LINCOLN GOODALE, A.M., M.D., Professor of Botany in Harvard University, being vol. II of *Gray's Botanical Text-Book.* New York and Chicago: Ivison, Blakeman, Taylor & Co. 1885. pp. 499.—Teachers of Botany especially will be glad to know that this long expected volume is at length provided to meet—as we trust it truly will—the demands of the higher instruction in their department. In making this announcement, it is not for us, at this time, to say more than this: that the volume, although moderate in size, is encyclopedic for the subject, is methodic and well-proportioned, is admirably illustrated, and will be thought to do credit to the series of which it forms a part. Thirty-five pages of Practical Exercises are bound up with the volume. This, and the Introduction of nearly as many pages on Histological Appliances, may give some idea of the pains that have been taken to make this book a *vade mecum* for the botanical laboratory. A. G.

6. *Rabenhorst's Kryptogamen-Flora von Deutschland, Oesterreich und der Schweiz. Vierter Band: Die Laubmoose,* von K. GUSTAV LIMPRICHT. Leipzig, E. Kummer. 1885.—This new edition, under the name of the late Dr. Rabenhorst's well known work, is truly a new one, of distinct volumes, by different authors, the *Pteridophyta* by Luerssen, the *Fungi* by DeBary, Rehm, and Winter, the Marine *Algæ* by Hauck. The latter is complete and has already been noticed here. Dr. Limpricht of Breslau now undertakes the Bryology; and two fascicles of his volume, of 128 pages, 8vo, are before us. More than half of these pages are devoted to structure and other general matters; but the second fascicle begins the Peat-Mosses, and carries them on to the twentieth species. The illustrations are in the letter-press and are excellent. A. G.

7. *On the Structure and Dehiscence of Anthers;* by LECLERC DU SABLON, (Ann. des sc. nat. bot., ser. 7, t. I, p. 97).—The author shows that the walls of the loculi of anthers differ as much in their histological characters as in their form. The cells of which the loculi are composed are very variously lignified. In some there is no appreciable deposit of lignin, but in others the amount is considerable and the distribution of the cells is characteristic.

It is known that under the influence of dryness, the lignified walls of cells contract less than do those which are unlignified. The unequal contraction of these different cells, as the anther approaches maturity, brings about a definite shrinking of the membrane in such a way as to break the loculus open in a determinate line of dehiscence. Some of the instances of adaptation adduced by the author are almost as striking as those which have long been known to occur in our dry fruits. G. L. G.

8. *Influence of strong sunlight on the vitality of Micrococcus.* M. DUCLAUX (Comptes Rendus, ci, p. 395).—Six forms of *Micrococcus* were placed under conditions most favorable to their rapid development in culture fluids, etc. In most cases exposure to sunlight for a few hours completely arrested all activity, and after fifteen to twenty hours all vitality was destroyed. G. L. G.

9. *On the Histology of Ascidia.*—HECKEL and JULES CHAREYRE have reëxamined the internal face of the pitchers of some of the species of Sarracenia, Nepenthes, etc. They recognize the existence of four regions of peculiar structure, all of which, as previously shown by Hooker, are concerned in the entrapping and probable utilization of insects. In the last paper by these investigators they state that the bottom of the pitcher of *Cephalotus fascicularis* is lined with a membrane which has multitudes of *aquiferous stomata.* From these exudes the liquid which serves to dispose of the captured insects.—*Comptes Rendus*, Sept. 8, and 21. G. L. G.

10. *Reserve carbohydrates in Fungi.*—LEO ERRARA (Comptes Rendus, ci, p. 391), shows that the food-reservoirs known as sclerotia possessed by certain fungi, present a remarkable similarity in their general behavior during storing and use, to the food reservoirs of the higher plants. It has been found by him possible to detect in these reservoirs nearly all the forms of stored-food of the higher plants;—for instance, oil (in *Claviceps purpurea*), glycogen (in *Peziza sclerotiorum*), and cellulose-thickening (in *Pachyma cocos*). During the so-called germination of the resting parts, these reservoirs exhibit chemical changes strictly comparable to those already recognized in Phanerogams. G. L. G.

III. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Elements of Projective Geometry;* by LUIGI CREMONA. Translated by CHARLES LEUDESDORF. Clarendon Press, Oxford, 1885.—Professor Sylvester and Professor Price have conferred a new favor upon mathematicians in inducing Professor Cremona, in connection with Mr. Leudesdorf, to issue an English edition of his Projective Geometry. The first edition of the original work was issued in 1872 and translations of it were very soon made in French and German. The present English translation is considerably enlarged and amended.

The work is not a descriptive geometry nor a treatise on perspective, but is an introduction to the methods of investigation now known under various names of Higher Geometry, Modern Geometry, Geometry of Position, etc. The knowledge required in a student of this work is that of elementary plane geometry, and the simplest elements of algebra. How little of the latter is needed is seen in the fact that ideal or imaginary points and lines, as well as the logical difficulties connected with them, are kept as much as possible in the background. The ideas of lines and points at infinity in the plane, of homology, of duality, of anharmonic ratios, of involution, of poles and polars, of polar reciprocal figures, and of foci, are developed in a manner to give to the student a clear apprehension of the several methods involved in these ideas and a power of using them. It is only a geometer of the highest rank who is capable of producing a first rate work of this character.

2. *National Academy of Sciences.*—A meeting of the National Academy was held at Albany, beginning with November 10, 1885. The following is a list of the papers entered to be read at the meeting. Those marked with an asterisk were read by invitation.

S. P. LANGLEY: On obscure heat.

J. S. BILLINGS: On a new form of craniaphore, for taking composite photographs.

A. S. PACKARD: On the Carboniferous Merostomatous fauna of America.

E. C. PICKERING: On stellar photography.

E. D. COPE: On two new forms of Polyodont and Gonorhynchid fishes from the Eocene of the Rocky Mountains.

C. H. F. PETERS: On certain stars observed by Flamsteed, and supposed to have disappeared.

JAMES HALL: Remarks upon the International Geological Congress, with a brief historical notice of the origin of the congress.

JAMES HALL: Notes on some points in the geology of the Mohawk Valley.

SIMON NEWCOMB: When shall the astronomical day begin?

J. W. POWELL: Remarks on the stone ruins of the Colorado and the Rio Grande.

A. GRAHAM BELL: Preliminary report on the investigation relating to hereditary deafness.

C. A. YOUNG: On the new star in the nebula of Andromeda.

C. H. F. PETERS: On the errors of star catalogues.

T. H. STAFFORD: On the formation of a Polar catalogue of stars.

JAMES HALL: Remarks upon the Lamellibranchiata fauna of the Devonian rocks of the State of New York, and the results of investigations made for the paleontology of the State.

*O. T. SHERMAN: On new lines in the spectra of certain stars.

*W. B. DWIGHT: Primordial rocks among the Wappinger Valley limestones, near Poughkeepsie, N. Y.

*J. A. LINTNER: On recent progress in economic entomology.

*C. H. PECK: The New York State herbarium.

*OTTO MEYER: On a Section through Southern Tertiaries.

OBITUARY.

DR. WILLIAM B. CARPENTER, the English Physiologist, died on the 10th of November last at the age of seventy-two years.

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